AMERICAN JOURNAL of PHYSICS

A Journal Devoted to the Instructional and Cultural Aspects of Physical Science

VOLUME 17, NUMBER 5 MAY, 1949

Lloyd William Taylor, 1893–1948

Then said Mr. Great-heart, "Who art thou?" The man made answer, saying, "I am one whose name is valiant-for-truth. I am a pilgrim, and am going to the Celestial City."

AFTER World War I, Lloyd Taylor, just back from two years of service as a Coast Artillery Corps lieutenant, entered a professor's office in the Ryerson Physical Laboratory of the University of Chicago and said bluntly: "I should like to get a doctor's degree in physics from the University of Chicago. However, I'm a 'schoolmarm' and not ashamed to admit it. Is there any chance of getting a degree under such circumstances?" The story is significant, for it portends the single-minded purpose, the courage, and the blunt honesty that characterized all of Lloyd Taylor's subsequent activities.

Taylor had taught for a year in an Iowa high school, and for two years in the mathematics and physics department of Grinnell College, after having received the A.B. from that institution in 1914. He had made up his mind exactly what he wanted to do after leaving the Army. The decision turned out to be a fortunate one for all of us in the teaching profession who have profited by his work and for the many young people who were his students.

In 1924, after serving for two years as a post-doctoral instructor at Chicago, Taylor went to Oberlin College as professor of physics and head of the department. From his first years there until the end, he pursued the development of the department with vigor and determination. The high standing of the department, the many successful major students, the present fine building—all are monuments to efforts of his originating in those first years.

It was also during these early years that he became convinced of the need for a new and broader approach to the study of liberal arts physics. His special interest, aside from general physics, was in optics, particularly spectroscopy. But his main and consuming professional interest was in the revision of general physics courses. The energy and determination with which he pursued this revision are well known to everyone who worked with him in one professional capacity or another during the past 20 years, and to the many teachers and students who are familiar with his textbooks and numerous published papers on liberal arts instruction.

He took the view that the enrichment of general courses through the use of historical and methodological material was beneficial for all liberal arts students, including those majoring in any science; and he knew that this enrichment could be had without sacrificing the basic physical content of the course. Several teachers in various colleges who employ this same approach in general physics have noticed that the students most vexed by it seem to be those who are majoring in some other natural science; they complain that there is too much emphasis on the "principles that did not work." Actually, what Taylor wanted the students to see was that the history of a science is, to a considerable extent, the history of the development of working

LLOYD WILLIAM TAYLOR, 1893–1948.

hypotheses, most of which finally have to be discarded.

A compelling faith in physics and the other sciences as mediums for general education is reflected in the following excerpts from his scholarly and highly original textbook, *Physics—The Pioneer Science:*

> The greatest significance of the study of physics is that it discloses one of the principal clues as to the way men think today···. While the practical aspects of physics are not to be despised, their significance is not so much in the multiplicity of inventions···as in the subtle conception which gave these gadgets birth and which is vastly encouraged by their use—man's confidence in his intellectual supremacy over nature.
>
> When science is viewed in this larger aspect, it is not its *effects*, profound and far-reaching though they are, that should be the primary interest of the student, but the nature of the instrument itself. It is utterly unique···. As a unified army, organized for a sustained assault upon the citadel of human ignorance, there has been nothing to compare with the sciences in the whole recorded development of human thought···.

Once Taylor was convinced through fact or well-considered opinion of the worth of a project, he pursued it relentlessly. The ability to do this, even in the face of persistently adverse criticism, was one of his strongest traits. His blunt manner in dealing with people having opinions different from his own sometimes led them to interpret his criticisms as personal in nature. Yet several of us can recall specific instances where he came to the defense of an individual after severely criticizing him on another issue. Taylor fought certain ideas and habits, but not those who possessed them. He was never able to see why people were sometimes offended by such tactics because he honestly felt that there was nothing personal in his criticisms.

As a teacher, Taylor, in his own estimation, was "good, substantial, but not outstanding." Yet some of his students say that he had the rare ability to perceive the difficulties of the individual student and to lead him, step by step, from the simple to the complex. He knew his students well and was friendly with them, and in such a way as to inspire their confidence and respect. Their future welfare was a matter of great concern to him, and it is generally agreed that he had remarkably good judgment as to what a graduate ought to do and where he could best go to realize on his potentialities. His students were extremely loyal to him.

One former student says that Taylor's oral delivery in the lecture room was "fearful and wonderful." This is easy to believe, for he had a tremendous vocabulary and a propensity for using the longest words in it. In his lectures he made such frequent use of technical terms that

the students had to learn their meanings in self-defense. Yet his style was often more suited to Victorian letters than to modern scientific discourse—a habit that apparently led some people on casual contact to regard him as somewhat pretentious. Actually, it was some of the very attributes leading to this misjudgment that, in final analysis, helped to account for the man's great strength.

Taylor's students were impressed by his attention to details in preparing demonstration experiments and by the sense for good theater that he displayed in presenting them. It was not that he employed the spectacular for its own sake. Rather, his demonstrations showed the effects of painstaking rehearsal and careful timing, with the object of keeping the attention of the audience centered always on the essential principles involved.

In addition to his teaching and his skillful management of the Oberlin department, Taylor was, as we well know, one of the most active and valued members of the American Association of Physics Teachers. He served as Vice President for one term and as President for two terms. He was a member of the Executive Committee for three different terms, and for several years was a representative of the Association on the Governing Board of the American Institute of Physics. During one year—1946—he worked on five committees of the Association, being chairman of three of them. In emergencies, he could be counted on to assume responsibilities that were not primarily his own, or that no one else was willing to assume, merely for the good of the cause and to keep things going. This was characteristic of the man, both in Association affairs and in his work at Oberlin.

He was a member of the first Board of Editors of the *American Journal of Physics*, serving from 1933 to 1936. He continually supported the journal, as a competent referee for many articles and as a member of the journal committee. Of the various articles that he contributed to the journal, the one of which he was perhaps justly most proud was "The Untold Story of the Telephone" [5, 243 (1937)]; it was based on extensive historical research and, according to those who have seen the documentary evidence, provides a strong case for the contentions he so ably advanced concerning Elisha Gray's role in the invention of the telephone.

It would be difficult to evaluate or understand Taylor, even in his role of physicist, without taking into account his activities in the broad affairs of the College and the community. Loyal as he was to his chosen field of physics, he never took a narrow view of the significance of the sciences as a part of a liberal education. To him the educational program of the College was to be regarded as a total enterprise. Departmental boundaries should be minimized, and the College should work towards a course of study that was a well-integrated whole. For a time he served as president of the faculty Social Science Discussion Group, and in activities such as this exhibited the same qualities of courage and enthusiastic conviction as he did within physics itself.

Doubtless at Oberlin, as in most liberal arts colleges, there were some faculty members in the humanities who regarded the sciences as the bane of our culture, and doubtless there were colleagues in the science division who hewed to the narrow line of "science is science." Anyone like Taylor, who held tenaciously to the middle ground, would have to bear the brunt of the resulting dissensions. But he was of the type who often could contribute to a fusion and reconciliation of the different interests. In the midst of dissension, he was usually the man with the lowest "temperature," and he could hold things together where others might well have given up.

But he was far from being passive or uninterested in anything in which he participated. Even in the sphere of public affairs, he found the time to exert leadership. President Stevenson, of Oberlin, said of him: "He was not content merely to talk about the responsibilities of citizenship; he set a fine example by doing something about them." Since 1942 he served as chairman of the Lorain County Republican Central Committee. For six years he was chairman of the board of trustees of his church. President Stevenson, in speaking of Taylor's firm belief in the influence which the college should exert in the sphere of religion, recalled a remark

of Taylor's in an address to the undergraduate body:

> And when I die, some long-suffering minister will have to officiate at my funeral. I am not disposed to have this embarrassing task thrust upon him against a background of indifference or hostility on my part.

Taylor often spoke in college chapel or in church. We are also told how, on many a late afternoon, he could be seen returning to the physics laboratory with his arms filled with apparatus: he had been giving a talk on some timely subject to a high school or other community group. Usually he would tax his strength and endurance to the utmost, whether it were in work or recreation. His last few years at Oberlin cannot have been easy for him, including, as they did, an extensive Navy instructional program during the war and subsequent college adjustments presenting situations that were not easy to manage. But because he approached these problems and all others with courage based on the strength of his convictions, life remained for him a continual exhilarating adventure to the very end. This spirit of the man is evidenced even in the circumstances leading to his tragic death on August 8, 1948, while on a mountain-climbing expedition, during a long-deferred vacation trip with Mrs. Taylor and their son to the home of their daughter and granddaughter in the State of Washington.

Lloyd Taylor had married Esther Elenora Bliss in 1917, shortly after he entered the Army. The people of Oberlin tell us how enthusiastically and constructively Mrs. Taylor worked along with her husband in all sorts of community activities. "Straightforward, honest, plain—quite ready to risk unpopularity in a good cause—they did their full part as community members and good neighbors." The married life of this couple seems to be epitomized by a remark of Mrs. Taylor's to the writer: "I am so glad that I often told Lloyd how exhilarating it was to be married to a man who always acted on conviction—it made life such an adventure."

At a memorial service at Oberlin on September 24, 1948, Mr. D. M. Love, Secretary of the College, aptly characterized Lloyd Taylor as a Valiant-for-truth, which was, he pointed out, a way of saying that Lloyd was a true scientist, a constructive and intelligent citizen who never shunned unpopular causes because of the fear of personal advantage, and a staunch churchman and Christian gentleman. Mr. Love closed his remarks with a quotation of which the following is a part:

> *After this it was noised abroad that Mr. Valiant-for-truth was sent for by a summons· · ·. Then said he, "I am going to my Father's; and though with great difficulty I have got hither, yet now I do not repent me of all the trouble I have been at to arrive where I am. My sword I give to him that shall succeed me in my pilgrimage, and my courage and skill to him that can get it· · ·. So he passed over, and all the trumpets sounded for him on the other side.*

The general effect of this impressive memorial service was one of triumph.—D. R.

Excerpt from a Minute of the General Faculty of Oberlin College, November 9, 1948.—In his [Taylor's] service to the College, to the community, and to teaching, three characteristics were prominent: conscientious attention to duty, great capacity for work, and courage. Willing to accept unpleasant tasks and to support unpopular causes, he was ever ready to give help and counsel to students, friends and colleagues. For him—truth, right and wrong, were not shadowy outlines; they were defined sharply and his decisions were clear cut. Once made, a decision was supported with characteristic vigor.

Mr. Taylor's professional achievements will be remembered by all students and teachers of physics. His colleagues, on the Oberlin Faculty, will remember above all the qualities that lay back of those achievements: executive ability, careful scholarship, sincerity, patience and integrity.

Cooperative Committee on the Teaching of Science and Mathematics
The Work of Lloyd W. Taylor

PROFESSOR LLOYD W. TAYLOR represented the American Institute of Physics for three years on the Cooperative Committee, a group sponsored by the American Association for the Advancement of Science to study the teaching of science and mathematics. On November 1, 1948, the members of the Committee sent the following letter to Mrs. Taylor:

> The members of the Cooperative Committee on the Teaching of Science and Mathematics were deeply grieved to learn in August of the accidental death of their friend and colleague, Dr. Lloyd W. Taylor.
>
> Dr. Taylor came to the committee in 1945 as the representative of the American Institute of Physics and served in that capacity to the time of his death.
>
> Dr. Taylor has been interested in increasing the effectiveness of science teaching, in promoting an understanding of science and the methods of science teaching. His broad view of the significance of science in education with recognition of the importance of the total educational program made him an exceedingly effective member of our committee.
>
> During the years 1945 to 1948 the committee published the fourth of a series of reports dealing with the broad problems of science teaching; participated extensively in the preparation of the report, "Science Course Content and Teaching Apparatus Used in the Schools and Colleges of the United States," which has since been distributed to the Ministers of Education of the war-devastated countries as an aid in the United Nations rehabilitation program; and prepared a report on "The Present Effectiveness of Our Schools in the Training of Scientists" at the request of the President's Scientific Research Board, which was published as a part of their total report. Professor Taylor was, because of his background and outlook, particularly well prepared to contribute effectively to this work; and we, his colleagues, will long remember how clearly and forcefully he would express himself, and how wholeheartedly he would enter upon the task at hand.
>
> This committee wishes to express to you, Mrs. Taylor, their deep gratitude for having had the privilege of counting Professor Taylor among its members and for having had the opportunity to work closely with him on matters which were his deepest concern. Please accept our sincere and heartfelt sympathy in his passing.

Committee members, signatories to the letter, are listed below with the societies they represent and the institutions with which they are affiliated.

American Association of Physics Teachers: K. LARK-HOROVITZ, *Purdue University*, GLEN W. WARNER, *Chicago City College;* American Astronomical Society: RALPH C. HUFFER, *Beloit College;* American Chemical Society: C. H. SORUM, *University of Wisconsin;* American Society of Zoologists: L. V. DOMM, *University of Chicago;* Botanical Society of America: GLENN W. BLAYDES, *Ohio State University;* Central Association of Science and Mathematics Teachers: ARTHUR O. BAKER, *Cleveland Board of Education;* Division of Chemical Education of the American Chemical Society: LAURENCE L. QUILL, *Michigan State College;* Executive Committee of the AAAS: E. C. STAKMAN, *University of Minnesota;* Geological Society of America: GEORGE A. THIEL, *University of Minnesota;* Mathematical Association of America: RALEIGH W. SCHORLING, *University of Michigan;* National Association of Biology Teachers: PREVO L. WHITAKER, *Indiana University;* National Association for Research in Science Teaching: G. P. CAHOON, *Ohio State University;* National Council of Teachers of Mathematics: E. H. C. HILDEBRANDT, *Northwestern University;* National Science Teachers Association: MORRIS MEISTER, *Bronx High School of Science.* Chairman: K. LARK-HOROVITZ, *Purdue University;* Secretary: R. W. LEFLER, *Purdue University.*

L. W. Taylor's Challenge to the Teacher

Otto Blüh
University of British Columbia, Vancouver, British Columbia

A LIBERAL education, according to Aristotle, is the education of a free man: an effort in learning and understanding with a view of developing one's own 'excellence.' In contrast to the antique state, based on slavery, the modern democracy does not permit the free man to live a life of leisure. He is also a worker, a technician, a specialist, and his education must necessarily include disciplines of practical value for his work, to make him 'efficient' in his work or profession. Our modern system of higher education, therefore, compromises by giving first an exclusive so-called liberal education without vocational bias, complementing it, later, by graduate studies devoted entirely to specialized training. The need for more extended scientific or technical knowledge and experience causes specialized studies to encroach upon the early college years, and the time spent on liberal studies is so reduced that it can hardly permit an education in the Aristotelian sense. Liberal education is increasingly relegated to the years of adolescence, to a period of life when the values inherent in a liberal education—the education of a free *man*—can hardly be apprehended.

The college and university teacher can do much to improve this situation by establishing a closer relationship between a vocational training and a future citizen's education; by fusing both together, and directing his studies towards both a well defined professional aim and an education of a more universal character. To be effective in this respect, scientific studies must embrace the study of the humanities in an integrated process of learning, and truly

> to formulate the story of physics in terms of the great human values out of which it grew and which inhere in it today more strongly than ever should constitute not a stricture on one's teaching but a magnificent opportunity.[1]

Many teachers of physics will feel that this means transgressing into fields which today are not generally recognized as the responsibilities of the physics instructor. But there is good reason to believe that in this respect physicists must change their points of view, and that they

> must be much more than mere specialists in the most technical of all sciences. They must inculcate breadth of view of their field and through it a comprehension of the nature of the rest of the intellectual enterprise.[2]

When Professor Taylor wrote these lines he certainly foresaw that as teachers

> we will have to admit that we are at present very inadequately trained to make this contribution. . . . We in the colleges are primarily subject-matter specialists, and only secondarily educators. This has in large measure been brought about by the adoption of the Ph.D.-fetish in higher education, together with the narrowness of the qualifications that graduate schools established for the doctorate.[3]

The disadvantage of an over-specialized training on the graduate level has fortunately been recognized in authoritative quarters,[4] and it can be expected that a more comprehensive historical and philosophical education will be included in any new graduate curriculum. Already now there exists in the minds of many physics teachers an acute awareness of the need for an integrated cultural science course with a definite historical bias for the nonscience majors, in order to make the students understand and appreciate the wider aspects and implications of physics.

That the treatment of the historical development has been accepted as an important element of the so-called cultural approach is due to a great extent to the educational initiative of Professor Lloyd W. Taylor, and particularly to the influence of his textbook *Physics, the Pioneer Science.*[1] The book may not have reached the circulation among students that the high qualities of this outstanding work would merit, but it has certainly made its mark, in the hand of instructors, on the teaching of physics in the United States and Canada, to an extent which

---

[1] L. W. Taylor, *Physics, the pioneer science* (Houghton-Mifflin Co., 1941), p. vii.

[2] L. W. Taylor, *Fundamental physics* (Houghton-Mifflin Co., 1943), Epilogue, p. 662.

[3] L. W. Taylor, *Science* **91**, 563 (1940).

[4] *Higher education for American democracy, vol. I. Establishing the goals*, Washington, D. C., 1947. James B. Conant, *On understanding science* (Yale Univ. Press, 1947.)

can hardly be properly assessed. It may, in the future, gain even greater importance when the current trend of "understanding science through the historical route" at all levels of instruction is realized.

Taylor of Oberlin was firm in his belief that there is a need to acquaint every college student with the achievements of science, and that there is good reason to choose physics as the science which has been most prominent. He very definitely preferred a 'cultural physics course' to a multi-science course about which he was rather sceptical. In his last letter (June 3, 1948) to the writer he said:

The proposals of Conant and others contemplate a radical change in science teaching. It is so radical that a minimum of 20 years would be required to prepare teachers and get the venture under way. . . . Granting that Conant's plan is the best ultimate object (of which I am not at all sure), some interim plan is necessary. Science appreciation courses of the single-science type, injecting a lot of history of the subject and some philosophy, do lie within the range of teachers now in service if they are willing to try them seriously.

Taylor had been long since against

the common practice [of segregating students] at the beginning of the year into two groups, presenting to the one group a purely technical course and to the other a purely cultural course.[5]

As he said at the Princeton conference:

Presented to all beginning students . . . [physics] enriches the early approach of majors and non-majors alike. It avoids the prejudicial implication that the broader aspects are of scant interest to the specialist.

But he knew well that

an amazing wall of resistance has been built up against experimentation in this field on the assumption that any such venture is necessarily in the direction of relaxation of intellectual standards.[6]

Hence he purposely set out to prove through his textbook that this prejudicial assumption has little substance. That he was successful in proving his point everybody will admit after only glancing through the purely technical pages of the book. As with any new venture, it was impossible to please everybody, and Taylor found himself, as he wrote in a letter to the author (July 1, 1941),

being caught between the cross-fires of two types of commentators: one who objects to the omission of certain subtopics (to make room for historical material) and another who considers the book to be too long.

Such criticism evidently ignored the main purpose of Taylor's writings. His intention in writing the book arose from an ardent desire to stimulate interest in the wider aspects of physics teaching at a time when the political scene of Europe showed only too crassly a situation which Taylor considered to be the ill effects of unbridled specialization. In the Epilogue to the war-time edition of his book he maintained that

the first step is the destruction of the power of those who are responsible for this relapse into intellectual barbarism. The next will be to furnish leadership in a return to the scientific ideal of disinterested search for truth.[7]

It is to be deeply regretted that it was denied to Professor Taylor, pioneer of the new approach to physics teaching, to participate in the great educational tasks lying ahead, in which the idea of disinterested research will gain strength from a study of the scientific development of the past.

The history of physics, to be sure, will obtain and retain its proper place in the educational curriculum only if its inclusion is based on a sincere belief in its value. A sprinkling of historical material, a few dates here, and a few anecdotes there, will not lead to any worth while improvement of physics instruction, nor will they help to make a significant step in the direction of higher educational aims.

Perhaps the most outstanding and noteworthy feature of Taylor's book is the deep sincerity with which he deals with the work of earlier scientists. There is nothing which would even faintly indicate the superior and belittling attitude in regard of the 'errors' of the scientific past. The gradual development of ideas is viewed as a general intellectual effort and fully acknowledged from the standpoint of the contemporary. Taylor shared the attitude of historical thinking with the great historian of physics, Ernst Mach, who in his time also desired to utilize the understanding of the science of the past as an educational instrument. Taylor was, like Mach, an intellectual liberal, and must have truly enjoyed reviving the history of the centuries of free

[5] *Am. Physics Teacher* 6, 317 (1938).
[6] Reference 3, p. 562.
[7] Reference 2, p. 662.

thought and of their personages. I think he must have felt like Mach when the latter wrote "All who have experienced, in part, in its literature, this wonderful emancipation of the human intellect, will feel during their whole lives a deep, elegiacal regret (longing) for the eighteenth century."[8]

The intellectual American heritage of Taylor was of a similar nature, enriched, I understand, by a forceful protestantism, which apparently tied him closely to the whole period from Newton to Faraday. His being spiritually anchored in the period of scientific and religious protestantism and liberal enlightenment was the reason, I am inclined to believe, that Professor Taylor in his book paid only relatively little attention to the science of antiquity.[9] This may have been nothing but a wise limitation, but it may have been well-nigh dictated by the preference of dealing with an age most congenial to him in thought and sentiment.

[8] E. Mach, *The science of mechanics* (Chicago, 1907), p. 459.

[9] This theme is elaborated by the author in a paper on Greek physics scheduled for early publication in the *American Journal of Physics.*

The Oberlin College Laboratory of Physics

C. E. Howe and F. G. Tucker
Oberlin College, Oberlin, Ohio

When Lloyd W. Taylor came to Oberlin in 1924, the Department of Physics occupied the top floor and basement of the principal classroom building. With characteristic energy he immediately started a program for improving and strengthening his department. First, he obtained special appropriations for the purchase of new apparatus for both the elementary and advanced laboratories. Two years later, he added to his staff. In 1930, he was asked to prepare plans for a new laboratory. These plans were prepared in detail and a system of numbering apparatus was devised which later proved to be very effective. Although these plans were never used owing to the depression, for the next ten years Taylor continued to keep an active file of important laboratory features and facilities. This information proved to be extremely useful when the present laboratory was built in 1941. In the words of President Stevenson of Oberlin:

> "The erection of the beautiful new physics building—just recently named The Wilbur and Orville Wright Laboratory of Physics—symbolized, in a material sense the consummation of Professor Taylor's never-flagging interest in the development of his department."

Since Taylor's primary interest was the improvement of the undergraduate teaching of physics, the new building differs from many recent laboratories in that it was designed primarily for instructional purposes rather than for research. A front view of the laboratory is shown in Fig. 1. The design is a modern interpretation of French Romanesque; the construction is reinforced concrete with exterior finish of Indiana limestone.

The dimensions of the building and some of the features of the interior arrangements are shown by the floor plan, Fig. 2. It will be noted that shops, generators, batteries, storage rooms, and two small classrooms are located on the ground floor. The first-year laboratory, large lecture room, apparatus rooms and offices are on the first floor. The advanced laboratories, department library, two offices and individual research rooms are on the second floor—an arrangement which has proved very satisfactory since it avoids the heavy traffic of crowds of elementary students on the stairs and in the upper corridors.

All floors in the classrooms, instructional laboratories and corridors are of asphalt tile. Illumination, except in the lecture room and apparatus rooms, is by fluorescent lamps. Corridors, classrooms, laboratories and offices have adequate acoustic treatment. One feature that has proved very serviceable is the incorporation of steel channel bars into the walls of all laboratories.

There are two horizontal bars, 3 ft 9 in. apart, which give a very convenient method of mounting apparatus. Another feature is the large number of small rooms in the Light and Atomic Physics Laboratory. This makes it possible for each student, or pair of students, to carry on experiments requiring darkness or special illumination without interfering with the work of others.

Attention is called to four features of the building: the electrical distribution system, the main lecture room, the instrument shop and the numbering and card-filing of apparatus. The laboratory electrical distribution system furnishes electrical facilities to every student position in the instructional laboratories and to every other room where the services are likely to be needed. A complete description[1] of this distribution system has already been published.

Lecture Room

The lecture room, approximately 56×34 ft, has a normal seating capacity of 130 in 10 rows of fixed seats with room for 30 additional chairs. The rows of seats have a horizontal separation of 41 inches, each row being on a higher level than that in front of it, the risers increasing progressively from $2\frac{1}{2}$ to 6 in. The progression was determined by the architect in consultation with the American Seating Co. and provides good view of the lecture desk. Figure 3 is a view of the lecture room.

The permanent lecture desk is 12 ft long and can be lengthened by two five-foot tables on rubber tired wheels. The lecture table area can be closed off from the main lecture room by folding doors giving a closed room 9 ft wide across the front of the lecture hall. A blackboard on the folding doors and a small portable desk make it possible to use the main room for classes even while demonstrations are being prepared in the small room.

Steel channel bars are mounted flush in the ceiling above the lecture table and in the wall back of it for both permanent and temporary mounting of equipment. Illumination for this area is provided by fluorescent lamps and can be supplemented by a number of adjustable spotlights mounted at the ceiling. The controls for these lights are incorporated in the electrical panel mounted along the apron of the table. This panel, together with the associated electrical facilities, is described in the article previously mentioned.

At the rear of the room are four projection

FIG. 1. Front view of Oberlin College Physics Laboratory.

[1] C. E. Howe, *Am. J. Physics* 13, 192 (1945).

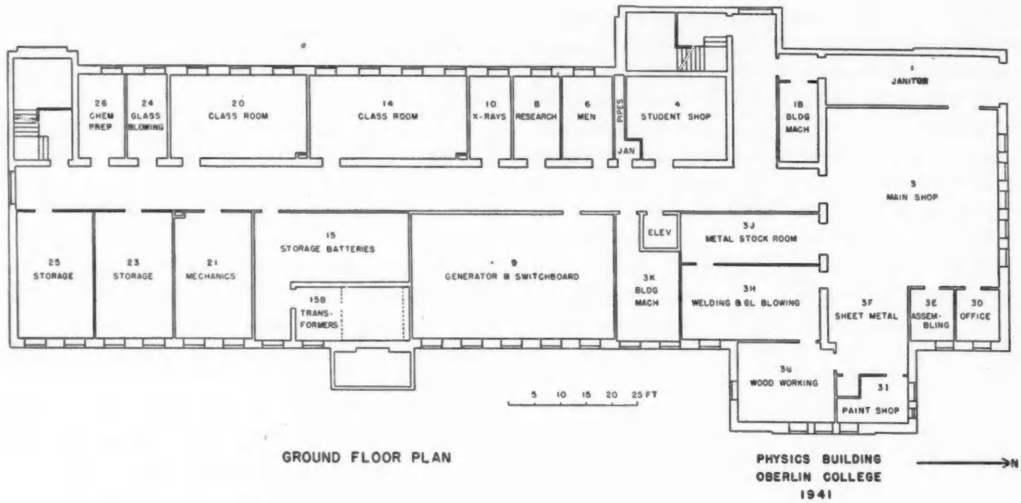

FIG. 2. Ground floor plan of Oberlin College Physics Laboratory.

lanterns; two for showing standard lantern slides, with a dissolving effect, and two for use with standard Weston projection meters. These projectors, as well as the room and lecture table lights, may be turned on and off from an operator's control board at the back of the room as well as from the lecture table control panel. No motor-operated light shades are needed since there are no windows, the room lighting and ventilation being provided artificially.

Instrument Shop

One of the most valuable features associated with the building is the instrument shop. Its services are available to any department of the College for the construction and repair of instructional and research equipment. Most of the work done by the shop is for the natural sciences, a large proportion of the time being spent in making new apparatus. In general, this new apparatus is designed for special problems and experiments since it is not the function of the shop to make equipment that can be bought from supply houses.

The repair of old equipment varies from the cleaning and painting of Bunsen burners for the chemistry department to the reconditioning of microscopes for the biological sciences. The production of new equipment ranges from the making of a simple wooden block for a demonstration experiment to the construction of a two-meter quartz spectrograph for advanced experimental work.

In general, detailed mechanical drawings are not furnished for the new equipment. Usually brief sketches are given to the shop men who proceed with construction after discussing the details with the designer. Frequent consultations expedite the work and assure the experimenter that the apparatus will satisfactorily perform the desired functions.

In order to render the services expected of it the shop is well supplied with tools and equipment for metal machining, sheet metal working, welding, heat treating, grinding, polishing, woodworking, painting, electroplating, engraving and glass blowing. The extent to which equipment is available may be illustrated by the case of metal machining. For the turning of metals there are four lathes; a medium-sized, general purpose lathe, a ten-inch tool room precision lathe, a large, heavy-duty, sixteen-inch lathe and a small turret lathe. Additional tools available are a universal milling machine with complete attachments, a shaper, a planer, three drill presses including a radial drill press, a vertical band saw and a horizontal band saw for cutting off stock up to twenty inches in width.

FIG. 3. The lecture room of Oberlin College Physics Laboratory.

The shop is located on the ground floor at the north end of the building and uses over ten percent of the useful floor space of the building. Most of the shop floor area is paved with wooden blocks placed on end. Adequate illumination is provided by fluorescent lights. Work benches placed along a row of windows receive light from the north.

This shop is the result of twenty years' growth. In 1924 when Professor Taylor came to Oberlin the shop facilities consisted of an old lathe, a drill press, a power hack saw, some small tools and the half-time services of one man. In 1928 a full-time instrument maker was employed. Piece by piece additional equipment was bought, some new and some used. The only time a special appropriation of any size was made for the purchase of new equipment was at the time the new building was erected. The budget allotted approximately $16,000 for new shop equipment.

It is difficult to evaluate the contribution the shop has made to the natural sciences. It is perhaps sufficient to say that it has permitted the teaching staff and the students to do more and better work than could otherwise have been done.

Labeling of Apparatus

"To each instructional laboratory is assigned a stockroom. Every piece of apparatus is labeled in such a way as completely to describe its location with reference to the stockroom in which it belongs. The label gives a number corresponding to the stockroom, case, shelf or tray, number of the individual item on the shelf or in the tray and, if the item consists of two or more pieces, a letter to identify the particular piece.[2]

"To illustrate, 105-21-B-2A means that the item is to be stored in stockroom 105, case 21, on shelf B, and is one part of item 2 on this shelf, 105-13-A2-1D indicates stockroom 105, case 13, tray 2 below shelf A, and one part of item 1 in this tray.

"The backgrounds of apparatus labels are enameled in color. A different color is assigned to the labels of each stockroom, thus giving a twofold method of identifying the proper location of apparatus."

The labels referred to above are aluminum labels, two in. long and $\frac{5}{8}$ in. wide, with the background of the label enameled in color. Slightly raised above the background is the inscription OBERLIN COLLEGE DEPARTMENT OF PHYSICS in two rows of letters. A space approximately $\frac{1}{4}$ in. by $1\frac{3}{4}$ in., is provided for the apparatus number.

[2] All of the passages quoted are from Section F, "Apparatus Administration," of Professor Taylor's 200-page Memoranda of Office Procedure Found Advantageous in the Department of Physics, Oberlin College.

This number is put on by a stamping machine similar to that used for making addressograph plates. The label is attached to the apparatus by means of small self-tapping screws.

When the nature of the equipment does not permit the use of these labels the equipment is hand numbered by other methods. A diamond pencil is used for numbering glassware. Other pieces of equipment are numbered by hand lettering with appropriate paint or by use of a vibrating etching tool. Many pieces of equipment made in the shop are numbered by direct engraving with a Gorton pantograph engraver.

Catalog Files

"The catalog of apparatus consists of four identical files. The original is hand written on paper stock. The other three are typed copies on heavy cards. The original cards form one of the two master files kept in the main office; this file is arranged in order of apparatus number.

"The second master file, consisting of typed cards, also kept in the main office, is arranged alphabetically.

"The third file is subdivided according to the laboratory to which each part pertains. For example, the section for demonstration apparatus is in 105. That for the first year laboratory apparatus is in 124, etc. Each section is arranged alphabetically.

"The fourth file is kept in the safe in the Treasurer's Office. It is subdivided in the same way as the third file."

The numbering system and filing arrangement have been satisfactory in every respect. This numbering system made the task of moving the equipment into the new building much easier than it would have been otherwise. Scarcely a piece of numbered equipment was misplaced.

The one exception to this numbering and filing system occurs in the case of lantern slides. The 3000 slides in the collection, obtained for the most part by Professor Taylor, are numbered and filed according to the Dewey decimal system of classification. A complete set of prints of the slides is mounted on 3×5 cards and filed for reference and for ease of selecting slides.

During the five years that this building has been in use there have been surprisingly few instances in which omissions of needed facilities have been noted or changes desired. This is due, to a large extent, to Lloyd Taylor's careful planning over a long period of years and to his insistence, at the time the building was designed, that a staff member be relieved of all teaching duties for a semester in order to work with the architect and building engineers.

If you think back over all the troublesome and annoying situations you have ever been in you will find that the trouble was usually caused by human beings, if not by yourself, then by someone else.

That, I think is what is wrong with the Government, if anything, and in this respect it is no worse or no better than all of our other group activities, namely, that it is run by people; just plain ordinary people, like you and me. . . .

Altogether too much has been said in the past about the bad features of working for the Government. It has been my experience that most of this stems from people who have either never worked for Government or did so many years ago. If any first-rate physicist doubts my word, let him come to work in the Government service. He will find that most of the bad things he may have heard are not true and that he will have the opportunity to work in splendidly equipped laboratories and libraries with a splendid group of associates who are just as able and devoted to the progress of science and to public service as any persons he could find anywhere.—E. U. CONDON.

Can We Fly to the Moon?

Joseph Himpan and Rudolf Reichel
110 Boulevard de Belgique, Le Vésinet (Seine et Oise), France

FROM time immemorial, as man has turned his eyes to the stars on a clear night, he has experienced the feeling of viewing a miracle. Innumerable worlds twinkle above him, arranged in the enormous distances of space. Astonishment, admiration, and awe come over him if he compares his everyday concepts of space and time with those that the heavens suggest to him. He is constrained to admit that all his previous efforts to comprehend three-dimensional space and time in his laboratory are inadequate to cope with the reality he observes in the skies. For sheer excitement and mystery, how can the most modern railroad, or motor car, or aircraft possibly compare with what he sees in the skies? Do they not creep along with a ridiculously small velocity compared to that which would be necessary to carry us in an acceptable time even as far as the nearest planet? But if we have the intention of traveling to another planet we must first be able to conquer the earth's gravitational field.

If we accept the universe in the form in which it appears, it becomes clear that interplanetary travel must be a very special activity—an activity, indeed, which would probably be the greatest in history. The fundamental question is, of course, whether it is possible, or not, to develop an interplanetary rocket missile. Some tens of years ago it was demonstrated that flying to the planets was at least a reasonable future possibility. However, as the years passed, the predicted interplanetary rocket did not materialize, although accurate calculations of the trajectory already existed. Rockets appropriate for interplanetary travel were readily developed only on paper. Many persons unfamiliar with the real nature of the problem speculated on the return of space pioneers after a lapse of years of travel, their rockets burdened with exotic acquisitions from other worlds, creating, on an unsubstantial basis, an atmosphere of mystery concerning the matter which was widely disseminated by books and the cinema.

A few years ago, a new fuel that might possibly aid interplanetary travel was created by the development of nuclear energy. Some authorities actually affirmed that now it would be easy to solve, in principle, the complete problem of energy, and that at least a journey to the moon would be expected in the near future. Just because the mere prospect of interplanetary travel is exciting and full of adventure, it is necessary to take a realistic point of view and ask "What are the facts of the matter, and what are the limits of present knowledge of the subject?" Meanwhile, as these thoughts run through our minds, the moon has risen, round and full. It stands large and silent on the horizon. Its appearance reminds us that of all possible interplanetary journeys a flight to it would be the simplest to achieve. In what follows, we shall subject this simplest interplanetary journey to a particularly critical examination.

Some General Considerations

A few preliminary remarks are necessary to give an insight into the theory of rocket propulsion and to summarize the problem of energy up to the date of the development of nuclear power. In order to have a correct idea of the magnitude of an interplanetary journey it is necessary to have in mind the latest achievements of the science of rocket flight. It can be taken for granted that in our present state of knowledge a departure from the earth can be accomplished only by means of a rocket motor, that derives all the energy needed for its propulsion and control from within itself. The largest long-range rocket built is the well-known V-2. It is a matter for sober reflection that the rocket motor of this missile, with a 25,000-kg thrust can work at most for a total time of 68 sec, and that therewith it is only able to bring a payload of 1000 kg over a distance of 330 km. From this simple consideration, it is clear that the conquest of the earth's gravitational field poses a particularly difficult technical problem. This fact loses little of its importance as a result of American experiments in which specially pre-

pared V-2 rockets attained, during vertical shootings, altitudes of 250 km and more.

If expense could be ignored, of course, long-range rockets with several stages could be produced today. With them multiples of the above mentioned altitudes and ranges would be realizable. A tentative design of such a multiple-stage rocket is shown in Fig. 1. Even without using all the latest technical developments, such a missile would be able to fly round the greater part of the earth's circumference. From this stage of development, admittedly the practical limit of present day knowledge and technique, there is still a long and difficult path to be beaten out before interplanetary travel is successfully accomplished.

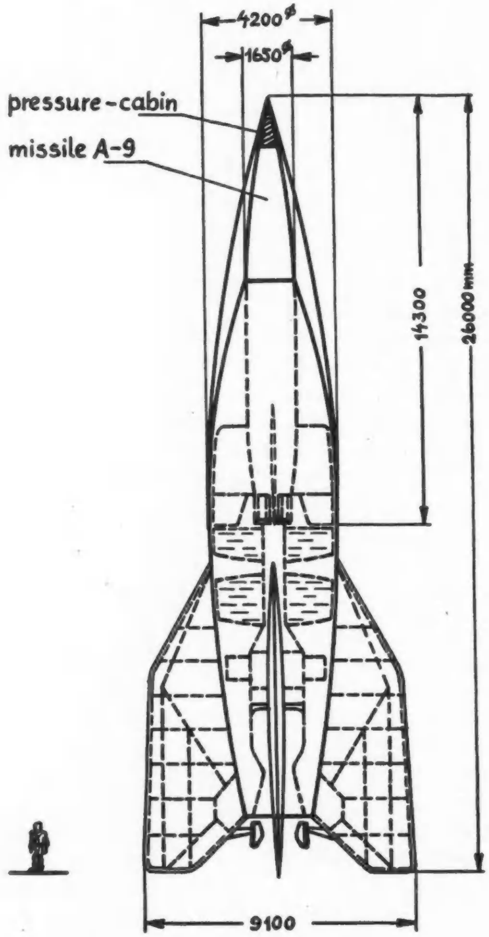

FIG. 1. Two-stage long-range rocket A-10, designed at Peenemunde before the end of World War II. The first stage (mother rocket) with a 100-ton thrust was to give the second stage (daughter rocket) an appropriate high initial velocity. This second stage, with dimensions and thrust similar to those of the 25-ton V-2, was winged in order to attain an increased range up to a maximum of 5400 km. For control purposes it was provided with a human pilot protected by a pressure cabin. Arrangements were made for his escape from the missile just before the end of the flight. The project was developed only as far as the unmanned launching tests of the second stage (A-9). Dimensions are in mm.

The proper conditions under which to use the rocket motor are in the vacuum of space at very high trajectory velocities. In fact, it can also be employed in every circumstance where a great effect is needed for short periods of time, even when optimum conditions cannot possibly exist. The development of the rocket motor has reached such a pitch, that its use in aviation is no longer considered inappropriate, if a special end is to be attained without regard to economy. The problem of energy limits the extension of this method of propulsion today. It does not appear feasible to improve the rocket motor's performance with the chemical (molecular) propellants in current use.

Simultaneously with the solution of the problem of liberating and controlling nuclear energy, there arose the idea of applying it to rocket propulsion, thereby creating a completely new propulsion system. Indeed, many physicists sincerely believe in the practicability of a rocket motor with high thrust, driven by pure nuclear power. The reason for this opinion is that to most physicists the field of rocket propulsion is unfamiliar, and they may be guided, therefore, by logic based on incomplete or irrelevant data.

On the assumption that the future development of rocket science from its state at the end of the recent war would continue at its previous rapid pace, the belief was widely held that a journey to the moon by man was now to be expected. Even in modern technical discussions of this kind, rocket propulsion problems are frequently solved only in an approximate manner, and in subsequent calculations propulsion values may be used that cannot be realized in practice.[1]

[1] Burgess, "Into Space," *Aeronautics*, Nov. 1946; Ackeret, "Zur Theorie der Raketen," *Helvetica Physica Acta* 19, 2 (1946); Ducrocq, "Le moteur nuclear" *L'Astronef*, June, 1946.

And since the propulsion problem is always the decisive one, then only after it is completely solved can the needed means of propulsion be placed at man's disposal.

The following discussions therefore emphasize a fundamental examination of the energy problem. It will be the aim of this article to exhibit the limits of performance of the rocket system—limits which are inherent in modern chemical propellants. For purposes of comparison, and to achieve the best possible approximation to theoretical values, typical examples or rocket techniques are used. These will be based on the performance of the best materials available today. In addition, the limits will be discussed on the basis of nuclear energy employing a working fluid. Later in this article the utilization of pure atomic energy will be considered and in connection with it an important fundamental law of the rocket motor will be derived.[2] A critical examination of the results will be made to find out if there is a possibility of human interplanetary travel today or in the future.

The Moon Rocket with Chemical Propellants

In this section we wish to design a moon rocket and to consider the calculations on which the design is based. Our starting point is the present-day state of rocket techniques. In the first place, the size of the rocket must be chosen in such a manner that we nearly attain the limit of technical possibilities. After that it will be a matter of interest to consider what payload can be sent with such a rocket to the moon. We can take for granted that the rocket must operate in several successive stages if it is to carry any useful mass to the moon. The calculation below will show that three stages are sufficient when one of the best chemical propellants that is known is used and when the rocket is constructed according to the most modern principles of design.

For all three stages a fuel oil of reduced chemical formula C_3H_4 is chosen with tetranitromethane, $C(NO_2)_4$, as an oxidizer. This propellant combination is one of the best available

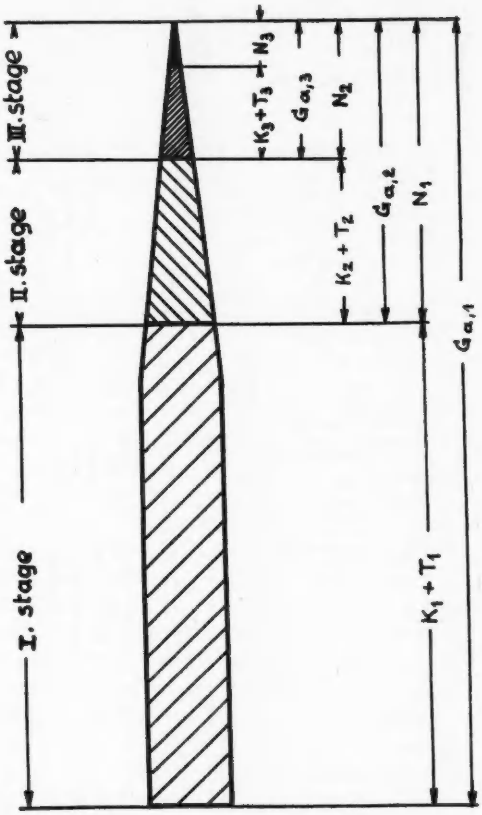

FIG. 2. Schematic diagram of the three-stage moon rocket.

today. It is much better energetically on account of its high density than, for example, the combination of liquid hydrogen and liquid oxygen. Moreover, it is to be noted that with the hydrogen-oxygen propellant the combustion chamber and nozzle cannot be cooled because both components are substances already at the boiling points under the anticipated conditions.

We shall assume that for all three stages the combustion pressure p_i is the same and is adjusted to be 20 kg/cm². As is well known, the effective exhaust velocity w_e increases as p_i increases. But it must not be forgotten that with increasing p_i goes an increase in the mass of the construction materials. Above a certain point this increase of mass nullifies the advantage of higher exhaust velocity. In addition, a greater value of p_i causes the combustion temperature

[2] See also Seifert, Mills, and Summerfield, "Physics of Rockets: Dynamics of Long Range Rockets," *Am. J. Physics* **15**, 255 (1947); H. A. Erikson, *Am. J. Physics* **14**, 374 (1946); Malina and Summerfield, *J. Aeronautical Sci.* **14**, 471 (1947).

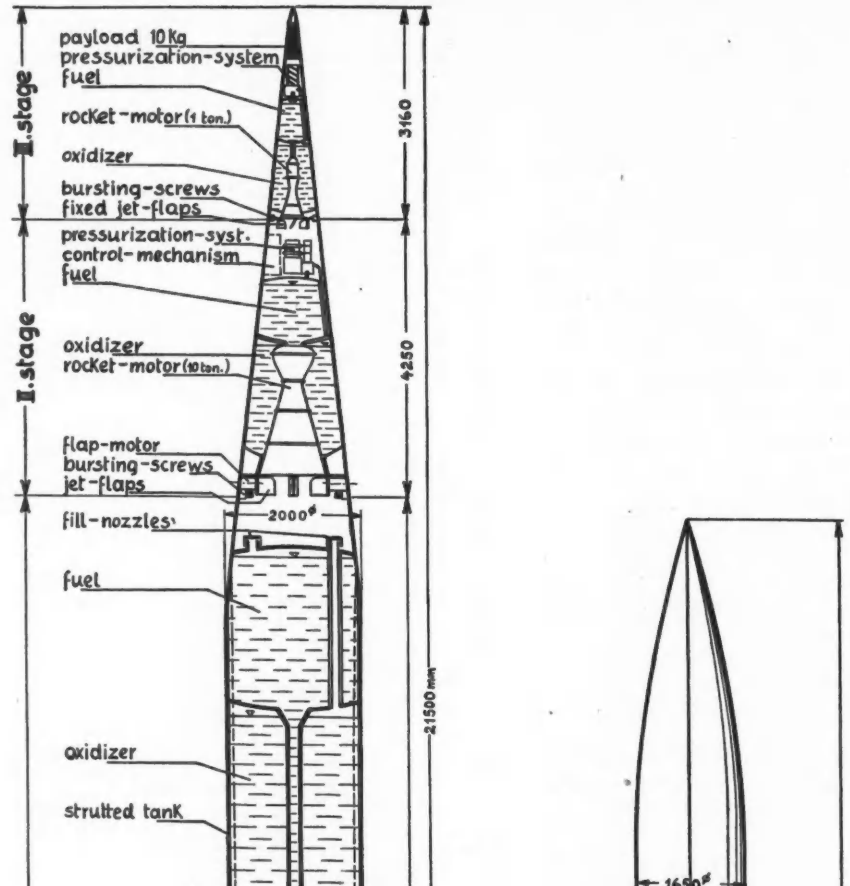

FIG. 3. Design of a 50-ton moon rocket with 10-kg payload. For comparison, the well known V-2 rocket is drawn on the right. Dimensions are in mm.

to be higher, thereby creating formidable, and in some cases insuperable technical problems in the cooling of the combustion chamber and nozzle. The value chosen for p_i is a result of experience. By choosing $p_i = 20$ kg/cm², the combustion temperature T_i is calculated to be 3260°K. The value 0.9 will be taken for the velocity factor χ, the ratio of the practical to the theoretical exhaust velocity. This represents a good average value attainable only with a rocket motor that is functioning well.

The most important data of our calculations are given below. The subscripts refer to the first, second, and third stages respectively. (See Fig. 2.)

Chosen values:

Total weight of the missile G_a:

$$G_{a1} = 50{,}000 \text{ kg}; \quad G_{a2} = 5000 \text{ kg}; \quad G_{a3} = 500 \text{ kg}.$$

Thrust R:

$$R_1 = 100{,}000 \text{ kg (sea level)}; \quad R_2 = 10{,}000 \text{ kg}; \quad R_3 = 1000 \text{ kg}.$$

Payload N:

$$N_1 = G_{a2} = 5000 \text{ kg}; \quad N_2 = G_{a3} = 500 \text{ kg}; \quad N_3 = ?; \text{ but } N_3 + K_3 = 95 \text{ kg},$$

where K is the mass of the empty missile.

Final pressure p_a of the expanded combustion gas (exit section):

$$p_{a1} = 0.8 \text{ kg/cm}^2; \quad p_{a2} = p_{a3} = 0.1 \text{ kg/cm}^2.$$

Ratio $T/(K+N) = \epsilon$: $\epsilon_1 = 4.5$; $\epsilon_2 = 4.0$; $\epsilon_3 = 4.26$, where T is the propellant weight.

By means of the above values the following were found[3] by calculation:

Effective exhaust velocity w_e of the combustion gas:

$$w_{e1} = 2160 \text{ m/sec}; \quad w_{e2} = w_{e3} = 2605 \text{ m/sec}.$$

Propellant weight T:

$$T_1 = 40{,}900 \text{ kg}; \quad T_2 = 4000 \text{ kg}; \quad T_3 = 405 \text{ kg}.$$

Rate of propellant consumption $\dot{G}$:

$$\dot{G}_1 = 454 \text{ kg/sec}; \quad \dot{G}_2 = 37.6 \text{ kg/sec}; \quad \dot{G}_3 = 3.76 \text{ kg/sec}.$$

[3] The details of the calculation are contained in a report "Remarques sur la théorie et la pratique de l'autopropulsion" by J. Himpan, prepared for the French Ministry of Defense (1946). Results are quoted by permission.

Duration of thrust t_e:

$$t_{e1} = 90 \text{ sec}; \quad t_{e2} = 106 \text{ sec}; \quad t_{e3} = 107.5 \text{ sec}.$$

Exit area F_a:

$$F_{a1} = 16{,}000 \text{ cm}^2; \quad F_{a2} = 7480 \text{ cm}^2; \quad F_{a3} = 748 \text{ cm}^2.$$

Empty weight K, excluding payload:

$$K_1 = 4100 \text{ kg}; \quad K_2 = 500 \text{ kg}; \quad K_3 = \ ?; \ (N_3 + K_3 = 95 \text{ kg}).$$

The data quoted above may be taken to mean that we consider that a missile of total weight 50,000 kg would justify the possible expense at the present time, and a like claim may be made for a rocket motor with 100,000-kg thrust at the previously named pressure and temperature. The assumed ϵ-values (for a V-2, ϵ is 2.43) are also achievable because we have on the one hand, a propellant of high specific gravity and on the other, a supply of special materials regardless of price. In connection with this point, it must be noted that, generally speaking, the larger the rocket, the higher the attainable value of ϵ. The justification for taking a larger value of ϵ for the smaller third stage than for the larger second stage will be discussed later. In the argument which follows, it will be assumed that in the first stage of operation the influence of air resistance is compensated by the pressure thrust R_{s1} (caused by diminution of the atmospheric pressure p_0 with altitude). For this stage we need not consider both these influences.

By using the values assumed or calculated above, it is possible to design a three-stage rocket according to the scheme shown in Fig. 3. The three stages are arranged one after the other in the usual manner. The largest body diameter is determined by the space required for the 100-ton rocket motor, whilst the very slender conical point makes possible a favorable mounting of the second and third stages.

To maintain the propellants at the right pressure in the first stage, turbine-driven pumps are employed because this system is the most efficient considering its weight. The vapor needed for the turbines can be prepared in a chemical decomposing device by methods already well worked out. The combustion chamber is surrounded by a jacket cooled by a continuous flow of the oxidizer (regenerative cooling). It has been

found by experience that the thermal behavior of these highly efficient combustion chambers is difficult to control, and it may be necessary to add a fuel-injection system for direct cooling. This principle has survived tests with the V-2 missile. The propellant tanks are partially formed by the supporting outer shell of the rocket and are appropriately strutted to absorb the thrust forces.

The lengths of the rocket motors for the second and third stages are relatively great because the Laval exhaust nozzles are designed by purely practical considerations for a back pressure of 0.1 kg/cm^2, corresponding to their working altitudes outside the atmosphere. To economize construction length, the combustion chambers are inside the oxidizer tanks, a feature of design that is thoroughly justified by experience. The compression of the propellant for the second and the third stages is done by a gas-pressurized system in which the gas is produced chemically. This method effects a considerable saving of weight and space. In the top of the rocket is the real payload which may, in the first instance, be thought of as a charge of flash powder.

The rocket is launched as follows: On account of the 100-ton thrust of the rocket motor, the take-off must be from a horizontal starting table, in a direction perpendicular to the earth's surface. Stabilization fins are not needed here, because the guiding of the missile (preservation of the vertical position) is done by jet flaps of graphite arranged in the exhaust jet. With this arrangement, only relatively feeble guidance momenta need be supplied, contrary to the requirements of the usual long range rocket in which the trajectory is curved. Consequently, in the control compartment of the first stage there are provided the instruments needed to transfer (by means of relays) the necessary correction signals to the flap motors. When the fuel contained in the first stage is exhausted, and the missile ceases to be driven, the second stage is freed from the first by the automatic ignition of explosive bolts. Simultaneously, the rocket motor of the second stage is started and the active part of the missile is further accelerated. In this case the control of the vertical position is accomplished by means of jet flaps, just as in the first stage.

At the end of the combustion period of the second stage the third stage is likewise freed from its parent by the breaking of explosive bolts. In this final stage the rocket has no guiding mechanism. Instead, stabilization is accomplished by rotation, because the graphite flaps which are fixed at an angle in the exhaust jet give a torque to the projectile.

The first and second stages are now falling back to the earth after having attained their proper altitudes. The third stage, having reached its final velocity, is capable of escaping from the earth's gravitational field, as will be shown in a later calculation. It is scarcely necessary to point out that a strike on the moon is possible only by proper consideration of the exact flying time and the corresponding position of our satellite.

As already mentioned, this moon rocket represents a missile realizable today only with the greatest technical effort. To obtain an accurate idea of the extent of such a project, one ought to be aware that the actual production of such a rocket does not, by itself, bring any positive result. The development of the 100-ton motor alone for the first stage demands an enormous expenditure. The experimental data, derived from test runs, that are necessary to bring such a motor to perfection are simply not available today. After purely mechanical difficulties of construction have been overcome, there is still needed a long series of experiments on the test stand, to check the reliability of the pumping systems, the guiding mechanism, the jet flaps, etc. A corresponding series of trials and corrections must be carried out for the second and third stages.

The test-stand experiments would undoubtedly involve great expense because the exhaust nozzles of the second and third stages are designed for an expansion to 0.1 kg/cm^2. No change in the expansion ratio is possible by reason of the inflexible arrangement of tanks. The only alternative would be to allow the exhaust gases to stream through a low pressure chamber in order to guarantee travelling without sudden shocks. In this case a reduction in pump performance by cooling of the exhaust gases would occur, accompanied by difficulties in maintaining the proper conditions in the vacuum chamber.

Only after the removal of many difficulties could a successful firing for an experimental launching be contemplated, combined with experimental separation and ignition of the single stages. One gets an idea of the needed expense when one considers how numerous are the possibilities of interruption of such a program. The separate stages must be tested individually and later in combination with the whole missile. At every test firing a missile will be lost. But such an expenditure of labor and material is inevitable before a rocket can be shot successfully to the moon.

A theoretical analysis[4] of the motion of a rocket up to the time of fuel burn-out leads to the following equations, where v_e and h_e are the final velocity and altitude, respectively. The motion is considered to be within the earth's gravitational field, and air resistance is neglected. The acceleration due to gravity is denoted by the symbol g_x which is taken, for the motion of the first stage, to have the value 9.81 m/sec^2.

$$v_e = w_e \ln(G_a/G_a - T) - g_x t_e. \quad (1)$$

$$h_e = w_e t_e - w_e(G_a - T/G) \ln(G_a/G_a - T) - \tfrac{1}{2} g_x t_e^2. \quad (2)$$

These two equations can be used simultaneously and we obtain the following values at fuel burn-out of the first stage:

$$v_{e1} = 2795 \text{ m/sec}, \quad h_{e1} = 80.8 \text{ km}.$$

To calculate the velocity and altitude of the second and third stages at fuel burn-out we must yet consider the thrust-effect, the so-called "pressure thrust." It is generally given by the expression:

$$R_s = (p_a - p_0) F_a. \quad (3)$$

Thus we obtain for the second stage, $R_{s2} = 748$ kg. This value corresponds to an equivalent increase of exhaust velocity $w_{s2} = 195$ m/sec. Instead of w_{e2}, it is now correct to substitute in both equations: $w_{e2} + w_{s2} = 2800$ m/sec. Consequently, setting $g_x = 8.83$ m/sec^2, we obtain $v_{e2} = 3565$ m/sec, and $h_{e2} = 127.5$ km. We find, further, for the total velocity v_g and total altitude h_g at fuel burn-out of the second stage:

$$v_{g2} = v_{e1} + v_{e2} = 6360 \text{ m/sec},$$

and

$$h_{g2} = h_{e1} + v_{e1} t_{e2} + h_{e2} = 505 \text{ km}.$$

By consideration of the pressure thrust it follows similarly for the third stage that $w_{e3} + w_{s3} = 2800$ m/sec and we obtain at fuel burn-out of that stage the following values, if we put $g_x = 7.65$ m/sec^2: $v_{e3} = 3830$ m/sec, $h_{e3} = 139$ km; and for the final velocity and altitude at fuel burn-out of the third stage we find

$$v_{g3} = v_{g2} + v_{e3} = 10{,}190 \text{ m/sec},$$

and

$$h_{g3} = h_{g2} + v_{e2} t_{e3} + h_{e3} = 1330 \text{ km}.$$

The kinetic energy A which a unit mass must have at a distance h_{g3} from the earth's surface in order to conquer the earth's gravitational field is

$$A = gr^2/(r + h_{g3}) = 5.16 \times 10^7 \text{ kg m}, \quad (4)$$

where r denotes the radius of the earth. At the end of its power drive the third stage actually possesses kinetic energy per unit mass amounting to $v_{g3}^2/2$ or 5.2×10^7 kg m. These figures show that the third rocket stage having a mass at burn-out given by $K_3 + N_3 = 95$ kg is able to overcome the earth's gravitational attraction and consequently can fly to the moon. Of paramount interest is the payload N_3 which can be taken along. The payload, of course, depends on the quantity K_3. It can be shown that the construction of stage three is difficult enough even if its mass be limited to 85 kg. There remains, therefore, a payload of 10 kg. The net result of all our discussion can be summarized as follows: By employing all the resources of present-day rocket techniques we can at enormous expense construct a three-stage rocket of total mass 50,000 kg with which a payload of approximately 10 kg can be carried to the moon.

In view of the meager success that can be achieved by this moon rocket, it may be thought that a new chemical propellant may be found which will change the predicted performance in a decisive manner. The answer, unfortunately, is an emphatic no. Obviously, more efficient propellants may be developed in time, but they cannot possibly change the order of magnitude

[4] These results are derived from new, unpublished calculations. Indications of the general basis for the calculation of these quantities can be found in "Physics of Rockets," by Seifert, Mills, and Summerfield, *Am. J. Physics* **15**, 121 (1947).

of the payload that can be carried successfully to the moon.

We must now return to our real problem, namely that of a manned interplanetary missile. We imagine that a human being intending to make a journey to the moon would plan to fly round the moon after the manner of a small satellite (without any consumption of energy) after he has left the earth's gravitational field. Such a plan would be the simplest, for the traveller would undoubtedly wish to return again. There would have to be an enclosed cabin for the crew, with automatic control of pressure, humidity, and temperature; and with space for oxygen-renewal equipment, stores, and control mechanism. All told, our voyager to the moon would certainly need several hundred kilos of equipment to keep him alive and comfortable, all of which we shall designate as the effective payload N_e. The missile of mass N_e must be capable of descending to the earth with a sufficiently low velocity to achieve a safe landing after the circumnavigation of the moon. Rockets are the only feasible methods of effecting this retardation, and they must have practically the same characteristics as were necessary for the ascent. The payload at the start of the flight from the earth would, therefore, have to be approximately $5\times10^3\ N_e$ kg. This value represents in our symbols the payload N_3 for the ascent. With such a payload it would be necessary to have a total rocket-weight $G_a=5\times10^3 \times5.10^3 N_e=2.5\times10^7 N_e$ kg. This result means that a journey by a human being to the moon and back is a very remote possibility by means of a rocket driven by chemical propellants.

The Moon Rocket with Atomic Energy

Although we have shown in the previous section that it is not practicable to accomplish a manned flight with chemical propellants to the moon and back, yet to answer completely our main question, "Can humans be expected to travel in interplanetary space by means of rocket-propulsion?," we must make a further examination of the problem, basing our considerations on the properties of the most energetic materials known. These materials are obviously those from which nuclear energy can be derived. In order to employ nuclear energy there are, in principle, two possibilities:

1) Liberated nuclear energy used to heat a working fluid which is thereby transformed into a gaseous state and ejected in the usual manner from a Laval exhaust nozzle.
2) Nuclear energy, liberated in a controlled chain reaction, used directly to produce the necessary thrust.

(*1*) *Atomic energy with a working fluid.*—The use of atomic energy in connection with another medium for the propulsion of the rocket is conceivable, for example, as follows: The medium is injected into a chamber to which is brought heat from an atomic energy source. There it is evaporated under pressure and highly superheated. The efflux of the nascent gases is directed through a Laval exhaust nozzle in much the same way as now occurs in the ordinary rocket motor.

One may ask what kind of performance is to be expected under the best conditions with such a motor? To obtain a formal comparison with present-day rocket propulsion, we start again with the moon rocket on which our former calculations were based. The only differences are that now the tanks are loaded with the working fluid (for example, water) and nuclear energy sources occupy the combustion chambers. In estimating the performance of the rocket motor it is now necessary to make some assumptions which are perhaps not beyond the limit of technical achievement. We suppose that:

a) the rocket motors can still be kept under complete thermal control especially with respect to cooling, if steam at $p_i=20$ kg/cm^2 is used at the temperature corresponding to that at which liquid hydrogen burns with liquid oxygen at the same pressure. This temperature is $T_i=3370$°K.
b) the weight of the proper atomic energy arrangement is so small as to be negligible.
c) the manufacture and the satisfactory working of an atomic energy producer is possible at the given temperature and pressure.

With the above mentioned ideal assumptions, calculations can be carried through as before to find what payload N_3 can now be sent to the moon. We obtain the following results with $\chi=0.9$:

$w_e=2860$ m/sec by an expansion from 20 to 0.8 kg/cm^2,
$w_e=3455$ m/sec by an expansion from 20 to 0.1 kg/cm^2.

Now it follows that for the first stage $T_1=27{,}800$ kg; $K_1+N_1=7820$ kg, and $G_{a1}=35{,}620$ kg. For the same thrust, $R_1=100$ tons, previously required, we need $\dot{G}_1=343$ kg/sec and $t_{e1}=81$ sec. By further calculation we now obtain $v_{e1}=350$ m/sec and $h_{e1}=100.8$ km.

Furthermore, for the second stage, $T_2=2720$ kg; $K_2+N_2=1000$ kg, and $G_{a2}=3720$ kg. Using the same thrust, $R_2=10$ tons, we find $\dot{G}_2=28.4$ kg/sec and $t_{e2}=95.5$ sec. Thus $v_{e2}=4040$ m/sec, $v_{g2}=7580$ m/sec, $h_{e2}=132$ km, and $h_{g2}=570$ km. For the third stage the mass will be limited to $G_{a3}=500$ kg. We may therefore choose $T_3=300$ kg, $K_3=85$ kg as fixed, $N_3=115$ kg, $R_3=1000$ kg as before. Thus $t_{e3}=105$ sec. Using these figures, further calculations give $v_{e3}=2630$ m/sec, $v_{g3}=10{,}210$ m/sec, $h_{e3}=109.5$ km, and $h_{g3}=1475$ km. Now we must make a check to see if the kinetic energy is sufficient to overcome the earth's gravitational field.

The kinetic energy possessed per unit mass by the third stage rocket is $\frac{1}{2}v_{g3}^2=5.2\times10^7$ kg m, a trifle more than the 5.16×10^7 kg m that is theoretically necessary according to Eq. (4).

In summary we may say that if atomic energy were used to vaporize water employed as a working fluid, then a rocket of initial weight 35,620 kg would be needed to shoot a 115-kg payload to the moon. Should we try to extrapolate this result to the case of a manned rocket capable of making the round trip, we meet the same difficulty as was encountered in the case of rockets using chemical propellants. The total initial mass would be distressingly large. At best this would be $310\times310N_e$ or about 10^5N_e, a practical impossibility. Of course, other working fluids might be utilized. However, no revolutionary change in the permissible mass ratio is to be expected by reason of the peculiar physical properties of any medium that might reasonably be used.

(*2*) ***Pure atomic energy.***[5]—If we are to consider the future possible direct application of nuclear energy to the propulsion of rockets, we must give free rein to the imagination in devising "atomic" motors. Let us therefore assume that a continuous liberation of nuclear energy is possible, and that it can be incorporated in some type of rocket motor. Such a motor would eject the products of reaction immediately and continuously in a well directed stream. In our imagination, it should operate in somewhat the same fashion as the rocket motor of today. Whether or not this goal will be realized in time cannot be decided now. But in our further examination of the problem we shall, without scruple, suppose it to be possible.

Some preliminary matters must be discussed first in order to clear the way for a formal treatment of the direct application of atomic energy to a rocket motor. At any time t while the rocket is in motion, the work that must be done in covering a very small distance ds is given by

$$dE=M(dv/dt)ds. \tag{5}$$

This work dE is performed by the element of mass dM thrust out at time t. The instantaneous energy content W' of this element dM is transformed into kinetic energy with a certain external efficiency η_a. Therefore we obtain

$$dE=\eta_a W'dM, \tag{6}$$

so that

$$M(dv/dt)ds=-\eta_a W'dM. \tag{7}$$

The minus sign in Eq. (7) is necessary because the mass M is diminishing during the motion. For η_a we can put

$$\eta_a=2v(2\eta_i gJH_T)^{\frac{1}{2}}/(2\eta_i gJH_T+v^2), \tag{8}$$

where η_i is the internal efficiency, J the mechanical equivalent of heat and H_T the heat of reaction of the propellants. For W' in kg m/kg we can now write

$$W'=(\eta_i gJH_T+v^2/2). \tag{9}$$

By combining Eq. (7) with Eqs. (8) and (9) we

---

[5] This section is abstracted, in the main, from former unpublished researches of J. Himpan. "Remarques sur la théorie et la pratique de l'auto-propulsion," June, 1946.

TABLE I. Heats of reaction of some rocket propellants, including molecular and nuclear varieties.

Propellant	H_T (kcal/kg)
Gun powder	6.70×10^2
Ethyl alcohol—HNO_3	1.26×10^3
Fuel oil—$C(NO_2)_4$	1.80×10^3
Benzene—liquid O_2	2.19×10^3
Liquid H_2—liquid O_2	3.80×10^3
${}_3{}^7Li(p,\alpha)_2{}^4He$	4.42×10^{10}
${}_1{}^2D(d,p)_1{}^3H$	2.27×10^{10}
${}_{13}{}^{27}Al(d,\alpha)_{12}{}^{25}Mg$	5.13×10^9

obtain

$$Mdv = -(2\eta_i gJH_T)^{\frac{1}{2}}dM. \tag{10}$$

This is the fundamental equation of rocket theory derived from the energy law (without any consideration of the influence of the earth's gravitational field). By integration of Eq. (1) it is found that

$$v_e = (2\eta_i gJH_T)^{\frac{1}{2}} \ln(M_a/M_e). \tag{11}$$

For M_e we can put

$$M_e = (K+N)/g, \tag{12}$$

and for M_a

$$M_a = (K+N+T)/g. \tag{13}$$

A combination of the last three equations yields

$$v_e = (2\eta_i gJH_T)^{\frac{1}{2}} \ln(1+\epsilon). \tag{14}$$

Putting Eq. (14) in logarithmic form, we may write

$$\ln(v_e) = \tfrac{1}{2}\ln(2\eta_i gJH_T) + \ln\ln(1+\epsilon). \tag{15}$$

If, now, ϵ is interpreted as a parameter, Eq. (15) represents a family of straight lines in logarithmic coordinates. When these are displayed as they are in Fig. 4, we have a chart that summarizes the possibilities and limitations of rocket propulsion. The values H_T of the various propellants are represented as vertical straight lines. Thus we can read off for each η_i and each ϵ the corresponding v_e which is of practical interest. By this method we can correlate the performance of different propellants and of different designs of rocket engines.

The rate of consumption of the propellant is given by Eq. (10). When we write bdt for dv in that equation, we obtain the expression

$$|dm/dt| = R/(2\eta_i gJH_T)^{\frac{1}{2}} \tag{16}$$

for the rate of consumption of the propellant

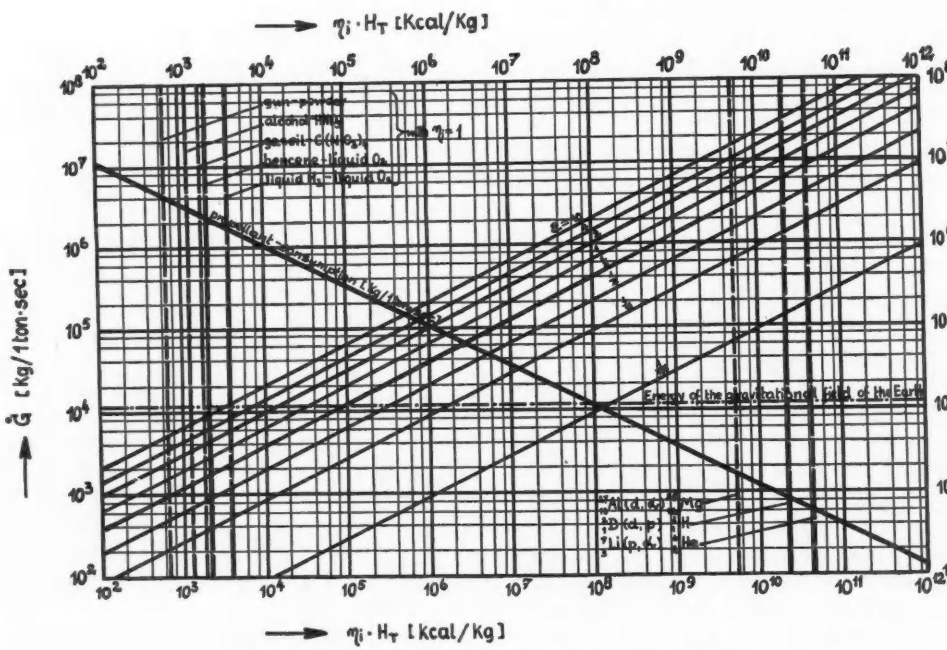

FIG. 4. Performance chart for rocket-propulsion devices.

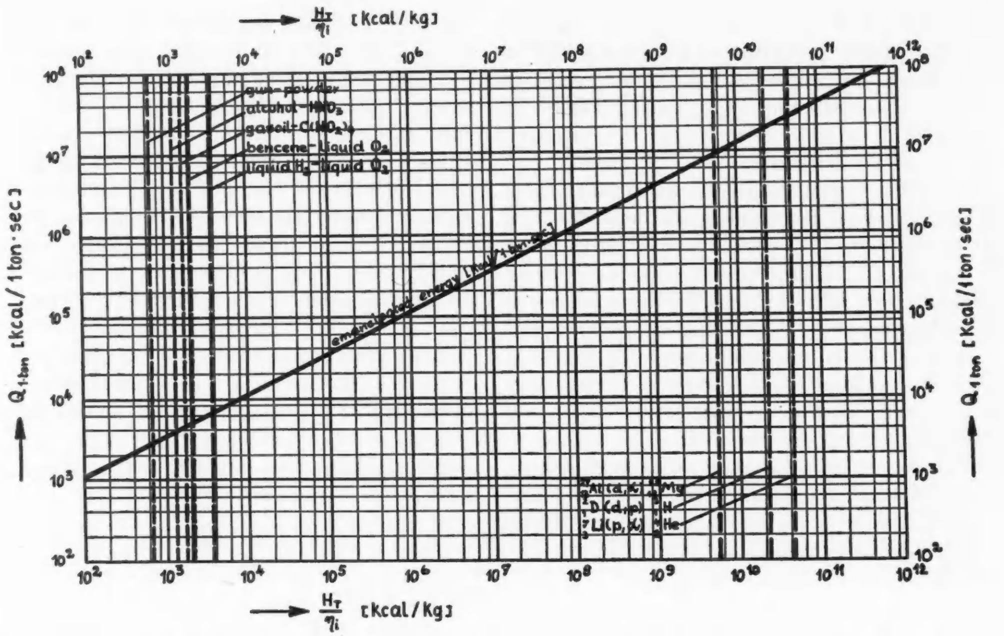

FIG. 5. Energy dissipation needed in a rocket motor for a 1-ton thrust as a function of the heat of reaction H_T of the propellant and of the internal efficiency η_i.

(metric slug/sec) for any particular thrust R, if R is in kg wt. The result may be written to give the rate of fuel consumption in kg/sec as follows:

$$\dot{G} = R(g/2\eta_i J H_T)^{\frac{1}{2}} = 0.1072R/(\eta_i H_T)^{\frac{1}{2}}. \quad (17)$$

The propellant consumption for 1-ton thrust is then:

$$\dot{G}_{1\,\text{ton}} = 107.2/(\eta_i H_T)^{\frac{1}{2}}(\text{kg/sec}). \quad (18)$$

Equations (15) and (18) are evaluated in Fig. 4. To emphasize the disparity between the results expected from molecular propellants and from possible nuclear fuels, curves are drawn for both types of energy sources. Table I gives the values of the heats of reaction of some propellants that might be considered. The atomic reactions that are included are to be considered as formal examples only. They are selected without any regard to practical application, since our ultimate aim is to show how the possible availability of nuclear energy affects our general problem.

It is easy to see from Fig. 4 that the atomic reactions give extremely favorable ϵ-values at the needed velocities of interplanetary rockets. If we consider, for example, more particularly the atomic reaction ${}_{13}{}^{27}\mathrm{Al}(d, \alpha)$ and assume for the purpose of formal comparison that we could obtain with it an internal efficiency equal to that previously considered for fuel oil-tetranitromethane (for $p_i = 20$ kg/cm² and $p_a = 0.8$ kg/cm², η_i is 30.9 percent), we get the following result: For a moon-rocket capable of making the round trip with a top speed of 22.4 km/sec, the necessary value of ϵ would be only about 1/160. This means that a 50-ton rocket would require only 312 kg of propellants. The propellants thus would make up only a very small fraction of the rocket's total weight—a most desirable and necessary requirement of every interplanetary space rocket. It is clear that in such circumstances it would even be possible to make technical modifications of the conventional design for the protection of a pilot's cabin against dangerous fission products. Many tons of material could be used without unduly restricting the weight of other necessary installations. When we consider further the needed fuel consumption, it turns out to be only 0.0027 kg/ton sec com-

pared with 4.54 kg/ton sec for the first stage of the chemically propelled rocket, according to the calculations above. All these facts seem to make an interplanetary space rocket possible in principle, if atomic energy can be employed directly as a propellant.

Finally, we must discuss an important aspect of rocket techniques, namely, the dependence of the energy output of rocket motors on the heat of reaction H_T and the actual internal efficiency η_i. By Eq. (18) it is seen that the consumption of propellant becomes less the higher the values of η_i and H_T. Our last calculations put us in such a favorable position that we may now omit any special consideration of the influence of the specific gravity of the fuel used.

We now ask what amount of energy Q must first be dissipated in the rocket motor in order to produce either a thrust R or a thrust of 1 ton. The result can be obtained directly if Eqs. (17) and (18) are multiplied by H_T.

For a thrust R, we find

$$Q=0.1072R(H_T/\eta_i)^{\frac{1}{2}}(\text{kcal/sec}), \qquad (19)$$

and for a 1-ton thrust

$$Q_{1\,\text{ton}}=107.2(H_T/\eta_i)^{\frac{1}{2}}(\text{kcal/ton sec}). \qquad (20)$$

The meaning of Eqs. (19) and (20) is twofold:

a) With the same thrust, the quantity of heat that must be dissipated in the rocket motor is higher the smaller the value of η_i. This result is trivial.

b) With the same thrust and the same efficiency η_i, the dissipation of heat by the rocket motor must be greater, the greater the heat of reaction H_T of the propellant. The increase is proportional to H_T. Hence, the propellant which should be considered as more favorable from the point of view of basic rocket theory (large w_e) becomes continually less favorable from the point of view of energy output when its heat of reaction increases.

The trend of Eq. (20) is plotted in Fig. 5. Returning to our formal comparison between the calculated moon rocket driven by a molecular propellant and that propelled by atomic energy, we obtain for the former, per 1-ton thrust, 8.2×10^3 kcal/ton sec of energy; and for the latter 1.38×10^7 kcal/ton sec. This means that it would be necessary to liberate 1685 times the energy in the nuclear rocket motor in comparison with the fuel oil-tetranitromethane rocket, in order to produce the same thrust. Equal internal efficiencies are, of course, assumed. Because we have already, in present-day rocket motors, attained nearly the technically controllable limit of energy dissipation, it must be accepted as extremely unlikely that we shall be able at any time to achieve a dissipation of energy amounting to over 1000 times the present limit. We are compelled to consider this last proven fact as the principal reason for concluding that a practical interplanetary space rocket propelled directly by atomic energy cannot be constructed in the foreseeable future.

Conclusion

In the world's scientific and technical literature the assertion is made continually that it would be possible, with the help of rocket propulsion, for man to fly out into interplanetary space to visit other stars and to return afterwards to the earth. However, space-ships have materialized from theoretical designs only in the present decade, as was pointed out at the beginning of this article. Following upon the success of rockets during World War II, and stimulated by the apparent availability of nuclear energy, the idea of interplanetary travel has appeared anew with increasing frequency.

This paper was written in an endeavor to give a plain statement of the real facts in this technical field. In reaching this objective, an investigation was made of the simplest of all possible interplanetary journeys, a trip to the moon. The necessary calculations and designs were outlined for a 50-ton moon rocket on the basis of the most advanced technical information now available. It was shown that it would be possible with very great expenditure of labor, materials, and money, to send a payload of 10 kg to the moon. It was demonstrated that it is not possible in principle to improve on this very low ratio of payload to total weight as long as chemical propellants are used. It was further deduced that a rocket capable of carrying a man to the moon and back would need to be of fantastic size and

weight—so large, indeed, that the project could be classed as impossible.

Our discussion revealed that the manufacture of an interplanetary rocket was not feasible principally because of the relatively low energy of even the best molecular propellants. Therefore, the possible use of nuclear energy was examined, first with an intermediate working fluid, and second with direct (but as yet undeveloped) use of nuclear fuel. The first alternative led, as before, to an impasse from which there was no practical escape.

In the final section, dealing with the possible direct application of atomic energy to interplanetary travel, we came again to the conclusion that in the light of present knowledge and technical achievement a round trip by a manned ship to the moon would be impossible, in spite of a very favorable ratio of fuel to total weight. The crucial obstacle in this case was found to be the impossibility of dissipating as much heat from the rocket motor as would be required to operate it under the conditions specified. The factor of improvement needed here appears to be between one and two thousand beyond the best that has been achieved up to the present time. The dream of human beings to fly to the stars must, as far as we can see, remain a dream.

Concerning the Teaching of Physics*

C. N. Wall
University of Minnesota, Minneapolis, Minnesota

THE fact that a corporation, devoted to the support and promotion of scientific research, has seen fit this past year to make substantial awards for "good teaching in physics" is worthy of note. The recognition by Research Corporation of the importance of effective teaching in the general welfare of our country represents such an enlightened point of view that it would indeed be difficult to overemphasize the action. Research Corporation is to be highly commended for taking this unusual step.

There seems to be a widespread opinion among many staff members of our institutions of higher learning that good research leads to promotion, while good teaching often goes unnoticed. That there is some basis for this opinion is hardly open to question. The reasons for this situation are not quite clear—until one poses the question: What is good teaching? It then becomes apparent that this is a difficult question with more than one answer. It raises the whole problem of educational objectives and their achievement and hence prevents an easy answer to the above question. As regards research the situation is not quite so complex. There are relatively few standards for measuring research; there are many standards for measuring teaching. Lack of agreement on the criteria for good teaching is in itself sufficient to prevent recognition of good teaching.

The Trytten report[1] in the July-August (1947) issue of *The American Journal of Physics* entitled "The Undergraduate Origin of Physics Ph.D.'s, 1936–1945" suggests one possible standard of measuring effective teaching in undergraduate physics. It is essentially a standard based upon the old Biblical quotation: "By their fruits, ye shall know them." In this report the undergraduate source institutions of all Ph.D.'s granted in physics in this country during the ten-year period from 1936 to 1945 are ascertained and classified. If it is assumed that one of the results of good teaching in physics is the encouragement of students to continue their work in physics even unto a doctor's degree, then this report serves to identify those undergraduate institutions where good teaching is presumably taking place.

In this report one finds, as you will recall, that a very large number of educational institutions have contributed little or nothing to the very

* Paper delivered at Luncheon Meeting of the University of Iowa Colloquium for College Physicists, Iowa City, Iowa, on Friday, June 11, 1948.

[1] M. H. Trytten, *Am. J. Physics* **15**, 330–3 (1947).

modest stream of Ph.D. candidates in physics during the ten-year period covered by the report. The sources of this stream lie in many, but by no means in all, of the big universities and technical institutions, and in a relatively small number of small colleges. For example, fourteen universities out of some 180 failed to produce a single candidate in the ten-year period. The others, in general, fell far short of producing candidates in the numbers one would expect on the basis of enrollment and strength of physics department. On the other hand, three out of four of the small colleges failed to supply any candidates whatever for the doctor's degree in physics during this period. The bulk of the Ph.D. candidates originating in the small colleges came from twenty-two colleges out of some 412 colleges. If one assumes that the general run of students in these colleges was roughly the same as to abilities, then one is forced to conclude that a very large number of potential physicists were lost during this period.

A second fact that emerges from the Trytten report is that, in the matter of contributing candidates for the doctor's degree in physics, the better small colleges outrank the better big universities on any proportional basis of enrollment or size of physics staff.

These facts will hardly surprise any of you who have had the experience of teaching physics both in a small college and in a big university. On the whole, the small college appears to provide a better undergraduate matrix than does the big university for inducing scholarly interests. If, in addition, the small college is fortunate enough to have competent teachers, the union is likely to be fruitful as far as the production of scholars on the undergraduate level is concerned. The splendid record of those twenty-two colleges mentioned before in supplying candidates for the doctor's degree in physics is due primarily, I think, to the happy combination of a small college and a good physics teacher. In this connection one automatically thinks of Knowlton at Reed, Edwards at Miami, Taylor at Oberlin, Barber at Ripon, Smith at DePauw, Roller at Wabash, as outstanding examples of this fruitful combination.

The fact that some 300 out of 400 small colleges in this country failed to supply even one candidate for a doctor's degree in physics during the ten-year period is, in my opinion, almost *prima facie* evidence that they did not have competent physics teachers on their faculties. Obviously many of these colleges, with their precarious financial state, were unable to bargain effectively against the large universities and industry for the all too few physicists turned out each year in this country. But, in addition to the factor of supply and demand, there is a second factor which frequently prevents colleges from securing the most competent teachers available. Entirely too many of these small colleges demand from their faculties every kind of qualification under the sun before competence in the subject to be taught. As a result we have physics courses in the small college being taught, on the side, by chemists, mathematicians, or even by retired ministers of the gospel. Such conditions are not likely to excite in the student much enthusiasm for physics.

When we turn to the large universities, we find that the combination of a competent physics staff and a big university is not necessarily a fruitful combination insofar as undergraduate physics is concerned. The element of size begins to play an increasingly important part. Elephantiasis sets in and radically reduces the chance of anyone getting an education.

I have taught physics in a small college where the entire student body numbered less than 600. I am now teaching at a big university where the enrollment reached 27,000 this past year. I have watched the size of my general physics classes grow from 25 students to ten or fifteen times that number, and the number of laboratory sections skyrocket from 2 to 150. In the course of this tremendous increase in numbers, I have been impressed with the fact that the number of problems involved in teaching physics effectively appears to be an exponential function of the size of the class.

The general physics lectures may be better in the large university than in the small college. The students may learn more physics. They may do better on the examinations, and the percentage of outstanding students may be larger. But when they are through with the course, they do not choose to go on with physics. In short, we can teach them physics, but we cannot make them want to be physicists. Entirely too few of our better undergraduate students "catch on fire" during their beginning course in physics.

After experiencing these extremes in size, I am not too surprised at the results. The *Minnesota Daily* in a recent editorial entitled "Adding It All Up" puts its finger on the problem of size when it states: "We've found that size and sheer numbers themselves present an almost insurmountable problem. For regardless of how often you say there are other factors, education is still an impersonal thing when you're sitting in a classroom with 250 other students—or attending a university with some 26,000 others." It is the impersonal character of education in the big universities that seems to block every effort to better things. Mark Hopkins must have realized this when he made the penetrating remark some fifty years ago that the ideal educational institution was Mark Hopkins on one end of a log and a student on the other. And before that Cardinal Newman in "The Idea of a University" had sagely commented: "A University is, according to the usual designation, an Alma Mater, ***knowing her children one by one***, not a foundry, or a mint, or a treadmill."[2] Surely we have come a long way from these ideals in our present educational institutions. What passes for education in our overcrowded colleges and universities is often a travesty on education. And in my opinion one ought to regard these institutions in the same manner that one regards a dancing bear. One is amazed not at how well the bear dances, one is amazed that he dances at all.

The size of the class is bound to influence the character of the relationship between the teacher and his students. With 20 or 30 students in the class, this relationship may be personal and vital; with 250 students in the class it must of necessity become impersonal. At some point one might as well replace the lecturer with a robot. I doubt that this exchange would produce any appreciable disturbance among the students, and I suspect that they would learn just as much physics.

As long as the student can see and hear what is going on down in front, why should the size of the class influence his reaction? Is it because, in losing his sense of any personal relationship with the teacher, he at the same time loses any sense of participation in the events which are taking place? I think this is quite likely to be the case. The student becomes a mere spectator instead of an active participant. This difference in point of view of the student is fundamental but I doubt that it can be detected by the ordinary tests and examinations. If the student is to get meaning and value out of a course in physics, or any other course for that matter, he must immerse himself in the stream, not stand watching on the bank. But this latter position appears to be precisely that taken by the great majority of students in a large lecture section.

In spite of the fact then that many of the large universities have excellent physics departments with able teachers, these institutions still find themselves unable to reach the undergraduate student in physics in a way that will make him want to continue his work in physics. In too many instances the process of educating him is reduced to a spray-gun method. He is sprayed with education in the same manner that a house is sprayed with paint. The most that can be said for this method is that it is a good way to paint a house.

I can see no easy solution to the problem of effective instruction in beginning physics at the universities that does not involve a thorough overhauling of many present day academic practices in these large institutions. And unfortunately many of these practices are just the ones most firmly rooted in the economic soil.

If, by this time, I have left the impression that these are no problems connected with the teaching of physics in a small college, I hasten to correct it. The difficulties encountered in this field may not be so obvious and so formidable as those met with in the universities, but they are likely to be more insidious and dangerous for the unwary.

A small college is usually small in more ways than one. One does not have to struggle with overgrown classes and laboratories but one may well have to contend with completely inadequate facilities of all kinds for proper instruction in physics. Quite frequently the physics professor in a college has first of all to convince the administration that the field of physics is one worthy of being supported and cultivated for its own sake before he has any chance of getting sufficient funds to build up the department. This is a job

[2] J. H. Cardinal Newman, *The idea of a university* (Longmans Green, New York, 1947), p. 128.

that has to be done. It is not likely to be done by anyone outside the department of physics. It probably has not been done in the majority of the small colleges in this country.

Given adequate facilities, ample funds, and a sympathetic administration, a competent physics teacher in a small college has one more pitfall to avoid, if he is to succeed in making the physics department a "going concern." This is the pitfall of stagnation, and it is a remarkably treacherous one. The very elements which make the college ideal for teaching also make it ideal for stagnation. The physics department is likely to be a one man department. There will be no recalcitrant colleagues in the department to take exception to your ideas about physics. The administration does not demand evidence of research ability as a basis for promotion. The college is probably located in a quiet village at a considerable distance from the distractions of large metropolitan areas. Only undergraduate physics courses have to be taught and hence the material in these courses is well known. All of these elements and many more make it extremely easy for the teacher and his department to settle into a fixed and moribund pattern from which there may be no recovery. Once *in* this state, the teacher soon loses any real interest and enthusiasm for the subject he is supposed to be teaching. His students, especially the better ones, are quick to sense this lack, for there is nothing so contagious in a small class as this enthusiasm of the teacher.

Now the best way that I know for a college physics teacher to fall into such a state of stagnation is for this teacher to devote his *entire* time to teaching and to the general "busy work" going on at the college. Paradoxical as it may seem, he must avoid this very natural inclination at all costs. It is necessary on occasion to get away from the affairs of the college in order to find out what other people are doing and thinking. It is of prime importance that he devote some portion of his time to research or to some other type of creative activity. This activity may take any one of a number of different forms. The important thing is that this work be original or creative in character and that it require and test the complete mental faculties of the teacher.

Only through such activity can the college teacher keep alive his interest in and his feeling for his chosen field, only in this manner can he enhance the quality of his instruction, only in this way can be excite in his students an interest in and an enthusiasm for physics. And it is this ability to excite that is the *sine qua non* of a successful teacher.

The Oersted Award

Elsewhere in this number will be found an account of the most recent presentation of the Oersted Medal, an annual award of the Association. The selection of the recipient of the Oersted Medal is a responsibility which rests chiefly upon the Committee on Awards whose members are the president and secretary of the Association, the two past-presidents of most recent incumbency and the last living recipient of the Oersted Medal.

The Committee on Awards invites nominations for the 1949 Oersted award. The requisite qualifications are fully stated upon the medal itself, which bears the inscription, "For notable contributions to the teaching of physics." Neither in the terms of the award nor in the historic practice of the Association does one find any restriction of eligibility by reason of geography, Association membership, field of interest in physics or precise kind of activity tending to justify the award.

The present Committee on Awards has available all correspondence and all transactions of its predecessors. No material ever submitted in support of a candidate will be overlooked in the 1949 deliberations, but opportunity is now given to extend and modernize past recommendations and to propose new names. Full documentation will strengthen such proposals and will be of the greatest assistance to the Committee in the discharge of this, their most important duty.

Committee correspondence should be sent to the chairman, PAUL KIRKPATRICK, *Physics Department, Stanford University, California.*

College Physical Science Courses in General Education

C. C. CLARK
School of Commerce, New York University, New York, New York

A PLACE in college curricula for a physics or a physical science course for general education is now fairly well established. In this modern age of radar, Nylon, Diesel trains, jet planes, atomic medicine, the public and the educator are keenly aware of the close relationship of physical science to modern life. Both are agreed that technical and professional courses for students who are training to work in physics or its related fields are needed in college and university curricula, perhaps more extensively today than ever before in the history of American education. Likewise, there is little difficulty in convincing the public that a nontechnical type of training in this field is advisable or desirable for all college students. Even in college educational circles the need for a course in physics or the physical sciences for purposes of general education to be taken by those students who are majoring in some field other than science is becoming widely recognized. The public statements and writings of some university presidents and college deans advocating such training, the work of the special committee of the American Association for the Advancement of Science to study this question, as well as the thinking of many physics, chemistry, geology, and astronomy professors in colleges and universities throughout the country, attest the fact that it is no longer a question of whether physics or physical science for general education is or is not worthy of a place in college curricula. I think that it is agreed that they should constitute a part of our college programs of study.

In my thinking about courses for general education I do not have in mind a program by which the student, following it exclusively, could obtain a college degree in general education. Rather, what I have in mind regarding the physical sciences are courses for the students who are majoring in some nonscience field, such as history, literature, business, in which they will do detailed and scholarly work and through which they will develop intellectual maturity. I feel strongly that physics and the other natural sciences have something of value to offer these students; but I do not think that the most value can be obtained by their taking a relatively small number of courses consisting of the detail and techniques necessary for those students who are to become physicists or other technical experts. Likewise, it is my judgment that students majoring in physics and other technical fields should have courses in the humanities, let us say history, for example, that gives them an insight into the major developments of world history, rather than one or two specialized courses such as those that include the detail necessary for students who are to become historians.

Based upon the assumption that physical sciences for general education are justified in college curricula, the main questions then become: What are the primary objectives of a physical science course? What shall its contents be? What methods of teaching shall be used?

Stated simply and briefly the objectives seem to me to be twofold: one is to provide the student with an understanding of some of the scientific phenomena around him and their applications to modern life, and the other is to give the student some insight into the scientific method as a basic type of reasoning in solving problems and in forming judgments.

It has been implied by many that the sole objective of physical science courses should be to impart scientific information so as to acquaint the student with some of the scientific aspects of his daily surroundings and experiences. In fact, most of the physical science courses that have been offered in colleges in the past seem to have been based upon this objective. The entire course consisted of an outline or survey of physical phenomena, with, in some instances, only inept references to the practical application of the principles and laws studied. Such courses, in my opinion, do not conform to the real need of physical science for general education, and it would be wiser if they were omitted as parts of a general education program from college curricula.

The dual character of the objectives I have stated is also somewhat at variance with one

compelling and influential idea regarding science training at the college level for nonscience majors; namely, the one so well expressed by James B. Conant.[1] He says, "In the last five years I have seen repeated examples of such bewilderment of laymen." (Dr. Conant is referring here to the layman's fundamental ignorance of what science can or cannot accomplish and his consequent bewilderment in the course of a discussion outlining a plan for a future investigation of a technical problem.) "If I am right in this diagnosis, and it is the fundamental premise of this book, the remedy does not lie in a greater dissemination of scientific information among nonscientists. . . . What is needed are methods for imparting some knowledge of the Tactics and Strategy of Science to those who are not scientists." President Conant further states that "What I propose is the establishment of one or more courses at the college level on the Tactics and Strategy of Science. The objective would be to give a greater degree of understanding of science by the close study of a relatively few historical examples of the development of science. . . . The course would not aim to teach science—not even the basic principles or simplest facts—though as a by-product considerable knowledge of certain sciences would be sure to follow."

It is not to be denied that colleges would be rendering valuable service to their students if they could offer training to nonscience majors that would lead them to an understanding of the tactics and strategy of science. However, I fear that a study of the historical development of science alone will not do this, at least with a majority of college students today.

How then shall the objectives I have stated be reached? They are to be achieved through the subject matter offered in the courses, and by the methods employed in its presentation. What are the basic principles to be followed in determining the content of the courses?

First, it is my judgment that our physical science courses should consist in part of scientific information with which the student is to become somewhat familiar. Bear in mind that the knowledge I am advocating is not the type that would attempt to make our students trained physics technicians; rather, it consists essentially of some fundamental scientific principles and an insight into some of the ways in which these principles have been applied to develop modern civilization.

In determining the material to be offered, we should avoid a course that is of the *outline* or *survey* type. The scope of man's knowledge in the physical sciences is too broad and too detailed to be grasped by an outline or survey study of the physical sciences. We must use, rather, a *selective* technique in determining the content of courses for nonscience students. In using this selective technique, one general principle I would advocate is to choose material wherein the practical applications of the physical sciences enter extensively into the students' daily life or have influenced greatly the development of American civilization.

Second, it is also my feeling that some material should be selected with the idea of giving the student an understanding of the scientific method as a way of thinking and reasoning, the manner in which scientists have attacked in the past and are now attacking scientific problems. Therefore, a second principle I would advocate in the selection of subject matter is to include the study of a few well chosen historical examples of the development of science, particularly those in which the attitudes of the times, the existing knowledge relating to the subject, and the work and methods of the investigators may be made reasonably clear. The historical examples chosen should be, if possible, those that have carried over into the present and are of interest to the student because of their application to modern life or because they still are in the forefront of scientific investigation.

Now, of course, just what areas of subject matter within these two categories are selected must be determined by the individual college so as to be best adapted to its own students and the faculty members giving the courses. But if the subject matter is selected on the basis of these two principles, it is my judgment that the courses will provide the student with some scientific knowledge and its useful applications, and with some understanding of how rational and impartial methods of investigation and thinking have solved difficult problems in the past and may be relied upon to solve such problems in the present and in the future.

And last, what methods should be used in

[1] James B. Conant, *On understanding science* (Yale University Press, 1947), pp. 12–17.

teaching physical science courses? Within the short space of this paper, only a few hints can be given.

After an area of subject matter has been selected and its essential elements designated, it seems to me that the first step should be to study the fundamental scientific principles involved, then to consider the manner and extent to which the application of these principles enters into modern life. In developing a concept of these principles and in studying their applications, the treatment should be in a relatively nontechnical manner and with the omission of extensive or higher mathematics. For example, suppose that the sources and uses of energy is one of the subject areas selected. Fuels and the chemical processes of oxidation constitute one of the sources of usable energy. Some of the simpler types of oxidation reactions together with the electron transfer in those reactions would, in my judgment, constitute sufficient depth to go into the principle of oxidation. Then we would study in some detail a number of instances in which the oxidation of fuels provides the energy of life and of most of the work of the world.

All essential elements of each subject should be illustrated with working demonstrations in the lecture or classroom sessions. These demonstrations should illustrate fundamental principles rather than technical detail, these principles shown clearly and simply, and in as spectacular and dramatic fashion as possible. In illustrating the relative amount of energy released in the oxidation of fuels in the internal combustion engine and the relative rate at which that oxidation takes place, for example the ignition of a proper mixture of hydrogen and oxygen in a rubber balloon by means of an electric spark from a spark plug is sure never to leave a student snoozing in class nor his attention wandering. I doubt that he will ever forget the fundamental concept of oxidation as a means for the release of energy, or fail to get some understanding of the source of power in his automobile engine.

It is well also to show working models or a representation of useful application of the principles when possible. Often to do this the demonstration equipment may be rather extensive or complicated, the technical operation of which is beyond the level of training offered; however, if the principle or application shown is clear and simplified the desired result will have been obtained. Such visualization not only greatly enhances the student's grasp of the subject matter, but also stimulates his interest in the field of science.

Even the historical developments should be illustrated to a considerable extent with working replicas of the apparatus used by the original investigators after the proper setting and difficulties of these investigators have been made clear by reading or class discussion. The demonstration of the classical experiments is always a delight to the instructor and usually of keen interest and enlightenment to the student.

I have had a part during the past fifteen years in teaching somewhat along these lines a physical science course that was organized at New York University twenty-two years ago and which has been given continuously since, and taken by several thousand students. We have varied the content and methods of the course several times, but as now constituted it follows the general pattern I have outlined.

While we have made no statistical study of the effects of our course on the students after they leave college, I think that we may have achieved at least two results: first, that most of the students have obtained enough understanding of science that when they see an automobile or television receiver or read about an atom bomb, for example, they at least do not dismiss the subject with the statement or thought "Science is miraculous!;" and second, that in solving their problems of business or citizenship they use some measure of rational and impartial thinking rather than depending upon hunches, long-shot chances, guesses, or biased opinion.

Experiment, directed by the disciplined imagination either of an individual or, better still, of a group of individuals of varied mental outlook, is able to achieve results which far transcend the imagination alone of the greatest philosopher.—LORD RUTHERFORD.

Some Contributions the Physics Laboratory Can Make to General Education

GWILYM E. OWEN
Antioch College, Yellow Springs, Ohio

SINCE the term *general education* has different meanings to different people, it will be necessary to give some indication of the way in which it is used in this discussion. By it the author means education whose object is *to develop the whole man or woman* and not primarily to develop along vocational lines. Its purpose is to help the individual to discover and develop all his potentialities as an individual and as a member of society. The definition does not exclude that part of education which has to do with vocational development, but in this discussion of the part the physics laboratory can play in general education, emphasis will be on the contribution such laboratory work can make to the education of those who do not study physics primarily as a step in their vocational program.

A physics course can make three major kinds of contributions to the general education of the individual. First, but not necessarily most important, it can assist him in getting a body of *knowledge* which will be interesting and useful to him as an individual, who needs to orient himself in a world which makes many of its impacts on him by means of physical phenomena, and as a social being, who must deal intelligently with the education of his children and with such problems as housing, conservation, the patent system, atomic energy, and many others. Second, it can help him develop certain *abilities* or *skills* which will be useful in all aspects of his living, vocational and nonvocational. In addition to minor technical skills such as weighing and measuring, it can help him develop the ability to read with understanding, to express himself with precision and clarity, and to develop the important abilities involved in the use of the scientific method: to formulate a question, to recognize assumptions, to apply general principles, to interpret data, to make tentative explanations or hypotheses, and to develop means or situations for testing an hypothesis. Third, it can help develop certain desirable *habits* or *attitudes*—a disposition to use the scientific method, a habit of looking for the assumptions underlying any logical argument, a habit of inquiry and observation, open-mindedness and intellectual honesty, and a faith that nature is orderly and predictable.

This discussion will not have to do with all these aspects of general education, but only with some which seem to have received too little attention, particularly those which can best be promoted in the laboratory. There is no doubt that the laboratory offers an excellent means for illustrating certain general principles, for demonstrating phenomena such as those of optics and electricity, for giving practice in the handling of numerical data, and for other purposes for which it is used in beginning courses. But how well does the ordinary laboratory experience contribute to the development of skill in applying the scientific method and of desirable attitudes and habits that should go with it? Consider that in the usual experiment someone else states the problem, develops the theory showing how general principles apply, outlines how the information is to be obtained and how it is to be interpreted and, in many cases, determines what the conclusion must be. Originality, open-mindedness, even intellectual honesty are not encouraged. The student is expected to follow the reasoning outlined for him and thereby learn something about the scientific method. The assumption must be that he cannot do this for himself. If so, the experiment does not help him much in learning to apply the method to his own problems. Moreover, the experiments are so much beyond the student's experience that he develops a lack of confidence, a feeling that the scientific method is something elaborate, mathematical, pertaining to an area of thought that is unfamiliar to him.

This criticism is not intended to condemn all experiments performed in the laboratory of the beginning physics course. It is assumed that they are assigned because they are valuable for purposes other than to help the student to practice the scientific method and to develop the scientific attitude. But some experiments should be designed so that the student can use the scientific method without help and which lead to results

which he can consider adequate. Such experiments can be set up, but only if they use equipment which is well within the student's ability and deal with phenomena which are not strange to him.

These experiments need not be entirely different, but they must be handled differently. For example, the simple pendulum experiment might be done by ignoring all the theory behind it and asking the student to determine how the period of a simple pendulum is related to its mass and length and *giving no further instructions*.[1] The instructor, with his considerable experience in the field, sometimes forgets the importance of letting the student have the thrill that comes from finding out for himself that the period does not depend upon the mass. He must be careful not to let his impatience to *get things done* blind him to the real progress the student makes. It is probably easier to follow a manual and show that the formula $T=2\pi(l/g)^{\frac{1}{2}}$ is experimentally verified than it is to arrive alone at the purely negative result that the period of a simple pendulum is not proportional to its length, but if the student accomplishes only that much, he has probably done himself more good than he would by a very careful verification of the formula. With a helpful suggestion here and there most students can see that the period varies as the square root of the length and can develop an empirical relation between these quantities. The time taken will try the patience of the instructor, but it is time well spent. The important aspects of this experiment are that the student gets no specific instructions so that he plans the experiment, that he is not bogged down with theory and explanation, that he does all the interpreting of results and arrives at his own conclusions without being told what is to be "expected," and finally, that he gets the feeling of having accomplished something on his own.

The following is an excellent experiment to use at the beginning of the course. It has the advantage of using only instruments which are familiar; students enjoy it and get the feeling that physics laboratory work can be interesting; it develops some measurement skills which can be useful to anyone in landscaping, photography, dress making, home decorating, etc., while the usual vernier caliper and micrometer experiment turns out to be useless to most nontechnical students and frustrating to many. Since this is the first experiment the instructions are somewhat detailed. The objectives are (1) to develop some skill in estimating and measuring distances, (2) to become familiar with the metric system, and (3) to practice estimating the precision of measurements.[2] The student is asked:

(1) to estimate the dimensions of the table at which he sits; then to measure it in English and metric units.

(2) to estimate the length and width of the room; to pace it out and get another estimate from that procedure; then, to measure it with a steel tape.

(3) having practiced pacing over a measured distance he is asked to pace the distance between two trees on the campus approximately 100 yards apart. He is then asked to state the result with an estimate of the precision and with some explanation of the reasons behind this estimate.

(4) to measure in any way he can the height of one of the college towers.

(5) to determine the straight line distance from a tree near the southeast corner of the science building to a bench mark near the northwest corner. The building being in the way, some vector or geometric method must be used.

The results of this experiment have been surprising in many ways: in a period of years students have estimated that their table is anywhere from 2 feet to 7 feet high. Nearly all students succeed in doing the experiment without much help except that got from other students and practically all get a feeling of accomplishment and success which is important with respect to their attitude towards future experiments.

Another experiment in which the student is

[1] Really the word "instructions" requires qualification, because the student should always be allowed to ask questions and be given helpful answers. He should not be told what to do; if he wants suggestions they should be given, preferably in a way that allows him a choice among several.

[2] There is an advantage in discussing precision first with reference to such a crude experiment because students have the feeling that the physicist always wants everything measured to the most extreme precision possible.

left to state his problem and then move towards a solution is introduced as follows:

I have received a letter from the department of buildings and grounds saying that they would like to paint the roof of x-building green (it has an aluminum-painted metal roof) and asking if that would affect the thermal conditions in the building. You are given several similar tin cans with large stoppers and thermometers. After experimenting with these you are to tell me what sort of reply to send. One can is aluminum-painted, another is green-over-aluminum. For your instruction I am also including three others for comparison purposes, one is shiny tin with no coating, one is painted with flat black, and one has a single layer of asbestos paper (such as is often used to cover hot air heating pipes in basements) wrapped around it.

Some of the advantages of this experiment are: (1) The problem is not clearly stated so that student has a chance to restate it. (2) The problem is so elementary that all can do something with it, but has ramifications enough to challenge the best of them. (3) The equipment needed is so simple that no student stands in awe of it. (4) All students get results which are accepted. (5) Some of the results are startling to the student and make him want to do further study on the subject.

What happens in the laboratory? Most students (the experiment is done in midwinter) consider the problem to be one having to do with loss of heat through the roof on cold days, so they fill the cans with warm water, put in the thermometers and observe the rate of cooling. Some do it in the laboratory with some attempt to shield the cans from drafts, and some think it more practical to put the cans outside where the normal wind effect takes place. Finally, most of them decide to do it both ways for comparison. Soon a student comes up with the idea that there is something wrong; the asbestos-covered can loses heat more rapidly than the shiny one. That gives the instructor a chance to suggest (1) that if the results look wrong you can try again and see if you have made some sort of error, and (2) that the experimental result when properly checked is to be accepted even though it may conflict with the result the student expected. One layer of asbestos paper over a shiny surface may increase the heat loss rather than diminish it. All students finally can report that heat leaks more readily from the green-painted can than the aluminum one, and incidentally, that the black can loses heat much more readily and the shiny one much less so. A few students go farther and estimate the loss of heat per square centimeter per second per degree difference in temperature between the can and the surrounding air.

A few students consider that the problem is to determine whether the green roof will cause the house to be too warm under the summer sun, so they put cold water in the cans and expose them to sunlight.

Such experiments as this can help the student develop skill in using the scientific method under circumstances which are nearly enough similar to problems in his own life that he can transfer that skill to his own problems. They can also help develop a scientific attitude; for example, to accept factual evidence even if it does not agree with his original prejudgment. This does not imply that all laboratory experiments should be designed in this way, but that some experiments should permit the student to do his own thinking.

I should be glad to do something towards knocking on the head the very prevalent view that Bohr's work is all juggling with numbers until they can be got to fit. I myself feel convinced that what I have called the 'h' *hypothesis is true, that is to say one will be able to build atoms out of* e, m, *and* h *and nothing else besides. Of the three variations of this hypothesis now going, Bohr's has far and away the most to recommend it, but very likely his special mechanism of angular momentum and so forth will be superceded.*—H. G. J. MOSELEY (1914).

The Effects of World War II on the Science of Physics*

Lee A. DuBridge
California Institute of Technology, Pasadena, California

I WANT to begin by saying that I realize as well as you do that I am a bold and probably a very foolish man to attempt to speak on the subject which was suggested by the committee responsible for this year's Richtmyer Address. It is clearly much too early to judge the ultimate effects of the war on anything. The immediate effect of the war on physics, as on many other things was, of course, to stop it. As we have witnessed the struggles of the past three years in getting physics into motion again it has often been unclear whether the phenomena we were observing were caused simply by the process of stopping and starting again or whether they were the peculiar consequences of the war itself. In any case, it is not very interesting to have a red traffic light force you to come to a stop. It is interesting, however, if something happened while you were stopped which made you decide to turn the corner instead of going straight ahead. The difficulty is that at the present time in physics we have been so busy getting started again that we can't be quite sure whether we are turning a corner or not—or even whether after we do get started we will be moving faster or slower than we were before.

Nevertheless, we must admit that things are different today from what they were in 1940. It is as though while our car stopped for the red light it was suddenly transformed from a 1940 to a 1949 model. Hence, we are now in the process of being thrilled over all the bright new chromium plated gadgets which we find on the new model. As we admire these new gadgets, however, we somehow miss some of the old ones which we keep reaching for and never find. Many of us find we rather liked the old bus pretty well after all. It certainly got us around where we wanted to go. Maybe the new one will, too, when we get used to it.

So today I am supposed to take a critical look at this 1949 model and see what it has that the old jalopy didn't have; to explain where the new features came from and why they are there and which ones we will discard after the novelty wears off.

Instead of plunging into an analysis of our 1949 car itself, however, I propose to take a look first at the driver. A car isn't much good without a driver. And physics wouldn't amount to anything without the physicists. They are the ones in the driver's seat—and as a matter of fact they also built the car. We'll obviously understand the car better if we understand the driver and builder and find out what has happened to him in the past nine years. In fact a good case could be made for giving exclusive attention to the driver and ignoring the car completely.

Our first inspection of the driver reveals the obvious fact that he is bigger and more prosperous than he once was. Today he cheerfully pays five dollars for a Physical Society dinner when ten years ago he regarded anything over one dollar and twenty-five cents as sheer robbery.

We notice another thing about him, too. He used to be able to tend to his business and not worry about the passing traffic. Today he acts as though he were half afraid that every traffic mix-up were all his own fault. Maybe he should have written an article or made a speech explaining to people that a car of the sort he builds and drives is a complicated and dangerous thing and ought to be handled with care. Sometimes his worry interferes with his own driving and we wonder if he is likely to have an accident himself. After all, he can't drive and build better cars if he doesn't give attention to the one he's got.

Finally, we observe one more thing about the driver. He seems to have become famous in recent years. He used to be able to go down the street quite unnoticed. But today everyone hails him as he goes past. Someone stops him at every other corner and asks him to give a talk on atomic energy at the Rotary Club next Tuesday.

But let's forget the analogy of the car driver and take a more serious look at the physicist of today and what the war has done to him.

* Richtmyer Address for Joint Meeting of American Association of Physics Teachers and the American Physical Society on January 28, 1949.

Referring again to his bigness, we are struck at once by a strange paradox. In spite of the fact that our colleges and universities practically ceased training new physicists for four years during the war there are far more physicists today than ever before. Look at the membership figures of the American Physical Society:

1938	—	3341
1942	—	4120
1946	—	5714
1948	—	7238
1949	—	8100.

What has happened? Are there really more physicists? Or are there just more people who claim to be physicists? Or is it just that more physicists now have the ten-dollar membership fee? My guess is that it's a little of all three. The war may have stopped the science of physics but it certainly greatly accelerated the activities of physicists. Of course, they all turned themselves into engineers—but they continued to call themselves physicists. And every lively person who had ever had a course beyond Physics I was dragged into a war laboratory, labeled a physicist, and put to work being an engineer. Pretty soon he even earned enough money to subscribe to the *Physical Review*. And the strange thing is that many of these men learned a lot of physics in the process. And today, possibly with the help of a year or two of post-war graduate work, they really are first-rate physicists. And our graduate schools are still all but choked with budding young physicists, many already members of the American Physical Society.

So let's write down result number one of the war: *It made more people want to be physicists.*

Now it is a very good thing that the number of physicists did grow so greatly for result number two of the war was this: *More people than ever before now want to hire more physicists.* The demand, indeed, increased much more rapidly than the supply. During and since the war government and industrial laboratories by the score increased their staffs of physicists, to say nothing of the increased demands of colleges and universities to take care of swollen enrollments and expanded research activities. Thus even with apparently twice as many physicists there still seem to be twice as many vacant jobs as there used to be.

The third effect of the war on physicists is this: *They are now better paid than ever before.* There are many who may argue that the increased salaries have not more than kept pace with inflation. But how well the physicist has fared compared with other professions may be resoundingly confirmed by inquiring of any professor, say, of English or philosophy.

A fourth effect of the war is that physicists now seem to have more fun than they used to. They not only enjoy their work, but they also enjoy each other. Some of the great war laboratories were rather like permanent meetings of the Physical Society. The many life-long friendships there formed, the magnificent *esprit de corps* which they developed is having, I believe, a most important effect on the spirit of physics.

But the most striking phenomenon of all—the one in fact which I think may ultimately be the most important—for physicists and for physics—is the increased prestige which the war and post-war developments brought. Do you remember the days—about half of you, of course, won't—when the American Institute of Physics used to call conferences and committee meetings to discuss how to improve the professional status of the physicist, how to bring the name "physicist" into public consciousness, how to persuade industries that physicists might be useful people? My, how we used to fret about these questions! Now I don't accuse the American Institute of Physics of bringing on the war, of course, but certainly no other event could have accomplished these objectives so thoroughly or so rapidly. I do admit that *cheaper* methods of accomplishing them might possibly have been thought up.

Now it is hardly necessary before this audience to review the war years in order to seek out the reasons why this greater prestige developed. The spectacular technical developments which came out of laboratories largely staffed by physicists provide an adequate answer.

But there is one paradox in this situation which I have already touched upon but which needs to be further emphasized if physicists are to avoid certain adverse consequences of their enhanced prestige. The physicists gained this prestige not because they were doing physics

but because they were doing engineering. Now prestige which descends upon a physicist because he is not a physicist is a dangerous kind of prestige. It is dangerous for two reasons—it may affect the physicist himself and it may affect the general public. The physicist himself may conclude that the way to retain this prestige is to keep on being an engineer. This, if widely followed, would be disastrous, indeed. Because physicists, prestige or no prestige, must keep on doing physics if we are to have any physics in the future or, indeed, any engineering. But though we should note this danger I regard it as a remote one. A glance at the program for this New York meeting will confirm the fact that physicists *are* being physicists again. Indeed, an increased interest in applied physics on the part of a substantial number of physicists may be one of the wholesome effects of the war.

The precarious position of the public attitude toward physicists, however, is a matter for greater concern. There are many people who now believe that physicists are closely akin to wizards and that they devote themselves solely to inventing things like radar sets that will make blind men see, rockets that will go to Jupiter, atomic bombs that will blow up the whole earth, and nuclear power plants that will transform the salty seas into oceans of nice fresh drinking water. (As a Californian, I spend half my time trying to squelch *that* one.)

Now what is the man in the street going to think of the exalted physicist if he finds, a couple of years hence, that all these things have not been forthcoming? There is bound to be some disillusionment. And *some* disillusionment will be a good thing. But we must do what we can to prevent disillusionment from turning into contempt or distrust. To this end we must strive at every opportunity to explain to the public what the role of the physicist really is: that he is not an inventor of gadgets or weapons but is one who seeks knowledge and understanding.

But if a portion of the prestige which physicists have acquired is based on a misunderstanding of their normal functions, a large and more important part of it is more soundly based. Well-informed people now know that the achievements of applied science during the war were based solely on the foundation which pure science had laid during the fifty years before the war, and the further strengthening of that foundation is essential to future progress and to future national welfare in peace or in war.

It is now also realized, in view of the fact that the war achievements in applied science were guided by men who were mostly pure scientists, that the success of the applied scientist or engineer in developing new applications of science may depend not so much upon his familiarity with current practices and techniques as upon his basic knowledge of science itself and his experience and skill in research. This fact has an important bearing on the current demand for physicists and on our philosophy of how to train engineers.

While I was preparing these remarks I ran across an address which George Ellery Hale delivered in Pasadena in 1919—after World War I. He said in part: "The experience during the war of the man of science has sometimes been confusing and it is possible that his responsibilities on the return to peace will not always be clearly recognized. Men who have previously devoted their lives to the advancement of knowledge have suddenly been called upon to solve practical problems of the greatest military or industrial importance. In attacking these new questions they have shown remarkable powers of adaptation and surprise has often been expressed that they could turn so readily from fundamental researches for the increase of knowledge to the most intensely practical undertakings.

"Some of these men, when seriously reflecting upon their responsibilities at the close of the war, have hesitated to return to their old tasks. They have often been applauded by those who know nothing of research, for their newly discovered ability to accomplish 'practical' results and to contribute in this obvious way to the public welfare. Or they have been offered by the industries salaries far in excess of those they received in the university or technical school. Which way shall they turn? How best may they serve the world?

"These questions have been clearly answered long since. Not only by students of science, but no less emphatically by great leaders of industry. One of them, for example, remarks, 'By every

means in our power, therefore, let us show our appreciation of pure science and let us forward the work of the pure scientists for they are the advance guard of civilization.'"

If any of us are worried about the problems we as physicists face in 1949 we may gain some comfort from the fact that the same problems were being faced by physicists just thirty years ago at the end of World War I.

So it appears we must thank the two world wars for bringing about a better understanding on the part of many of the place of science in civilization and the role of scientists in extending and applying man's knowledge. This has resulted in a greater public appreciation of and a greater prestige for science and scientists.

Now why is this important? What difference does it make to physics or to physicists whether there is understanding or appreciation on the part of the public?

The answer, of course, is that basic science depends for its existence on the support of the public. Whatever the immediate agency which pays for research, be it private, state or federal, some section of the public must be convinced of its value. An industry must convince its stockholders, a private university its donors, a state university its legislature and a federal agency must convince the Bureau of the Budget, Congress, and the taxpayers. Clearly the greater the appreciation and understanding on the part of the public of the function and the value of the work of the physicist, the more adequately in the long run will his research and his educational activities be supported, the more physicists will be in demand, and the more qualified young men will want to enter and will find a place in the field, and the more physics will be done. It is a challenge and an opportunity to keep this war-born prestige at the highest possible level.

I now touch lightly on another effect of the war on physicists, namely, on the delicate question of their new political activities. I shall not go so far as to commit myself as to whether this is a good effect of the war or a bad one. But it certainly is an effect. We must admit that some of our colleagues have declaimed a bit loudly on subjects outside their field of competence. But we must also admit that there have been political questions on which the voice of the scientist needed to be heard. The challenge here is that we as a group help maintain our hard-won public prestige and our value to the community by being as objective, as cautious, and as dignified in our political utterances as in our scientific ones.

So much for what the war has done to the physicist. What has been the effect on physics itself? Many people, of course, confuse the great contributions made by physicists with great contributions to physics. They assume that under the stimulus of war, and with the money and organizations which war made possible, the science of physics moved forward in great strides, accomplishing as much in five years as would normally take twenty-five.

Physicists know, of course, that this is nonsense. We did not have time for making many basic advances in physics during the war. Only those particular investigations which were directly related to, or necessary for, the design of some military weapon could be undertaken.

That we learned a great deal about the structure and properties of certain nuclei, about the behavior of electromagnetic waves under certain conditions, and about a variety of other matters cannot, of course, be denied. But as far as basic new knowledge and understanding of physical phenomena are concerned the war years must be chalked up as almost a dead loss.

And yet since the war we must admit that experimental physics has surged forward at a rate seldom if ever equalled before. To what can we attribute this?

There are, of course, many factors. There was the joy and enthusiasm with which most physicists returned to their basic studies. There was a reservoir of new ideas which piled up in the minds of many physicists during the war which could be tried out only when the war was over. There was a new confidence among the physicists, born of their new prestige. There were more physicists at work. There was more money available to them for their work.

But I think the major and certainly more permanent contribution of the war years to this present productive era was the vast collection of new experimental techniques and tools to which the war gave birth. There is no need to enumerate these to this group which is so busily engaged in using them. I will mention only two general

classes: electronic techniques and the new tools for investigations in nuclear physics.

Electronic equipment of a complexity and versatility previously undreamed of, at least in physics departments, is now found in profusion in every laboratory. Ten years ago most of us would have been unable to design or use a circuit which contained more than, say, two vacuum tubes. But today there are hundreds of physicists who throw vacuum tubes around like peanuts. They have added speed and precision to hundreds of physical measurements, and have made new experiments possible which previously could not have been thought of. Microwave techniques, pulse techniques, radiation counting and measuring techniques, charged-particle accelerating techniques, computing machines, timing, indicating, and recording equipment of an amazing and bewildering variety, are now employed in nearly every major research enterprise. The laboratory glassblower of the old days has given way to a room full of electronic technicians. Every field of physics from nuclear physics to astrophysics is benefiting from this development.

Then, too, we have all the tools for nuclear physics which resulted from the atomic energy project. We have neutron sources of fantastic intensities. We practically measure neutrons now by the pound. We have radioactive sources of a great variety and some of unbelievable intensity. We can obtain separated stable isotopes in amounts thought impossible a few years ago. All of these things, and others too, have greatly accelerated the progress of physics research.

At the same time, we should not forget that important discoveries have also been made with such simple things as a photographic plate or a molecular beam apparatus which long antedates the war. Ideas rather than techniques are still the prime movers in the physics laboratory.

But new techniques can open up new areas for the exploitation of ideas. It is these new areas of investigation which are the significant result of the new tools and techniques. As examples of such new areas of great interest we may mention microwave spectroscopy, neutron spectroscopy, nuclear induction, the exploration of cosmic rays in rockets, studies with new high speed, high resolution counting circuits, studies with new isotope separation, or mass spectrometer techniques and so on.

Of the 91 papers and letters published in the two November 1948 issues of the *Physical Review*, 21 (that is 23 percent) may be classed as concerned with new research areas opened up by the introduction of wartime techniques. Many others, of course, made significant use of many war-born techniques.

Aside from new areas of research opened up by wartime developments there has also been a shift of interest and of emphasis. Physicists are often accused of being fickle in their scientific loves, shifting in droves from one field to another as new discoveries uncovered unsuspected glamour. But the history of science has not been one of steady uniform advance simultaneously on all fronts. Rather it has been one of conquering one salient after another, capitalizing promptly on each new break-through. Let us hope that no superplanning committee ever attempts to decree otherwise.

To many it may appear as though physicists today were over-enamoured with the field of nucleonics to the neglect of other areas. But physicists know that nuclear science is not only a fascinating, but also an almost all-inclusive subject. Its ramifications extend to many unsuspected areas, from solid state physics to cosmology and deep into the realms of chemistry, geology, and biology. Furthermore, it is a field teeming with new possibilities, open to attack from many directions using many new tools and techniques. It is proper and desirable that this salient be extended to the limit.

But there are plenty of other areas which have been opened up to further study. Never has a young physicist had so many attractive lines of attack open to him. To those who fear that the war has narrowed the attention of physicists too sharply to nuclear physics the reply is that the broadening of nuclear science and the opening up of the other new areas already mentioned has indeed had just the opposite effect.

We come now to what is perhaps the most obvious and striking, though possibly not the most important, difference between postwar and prewar physics—cash!

To attempt to evaluate the effect on our science of increased financial support is a most

difficult matter indeed. I would like to list a few of the complicating factors:

1. It is difficult to get figures on how much the additional funds amount to.

2. Much of the increased funds have not gone to enlarge and strengthen prewar laboratories but to create new ones of a type unknown before the war, such as the Oak Ridge, Brookhaven, and Argonne Laboratories, for example. (I do not say this is wrong—quite the contrary. But it has changed the picture sufficiently so as to make comparison difficult.)

3. It is hard to estimate to what extent increased budgets must be discounted because of greatly increased costs. My guess is that $10,000 today will yield no more physics than $2000 to $4000 would yield before the war, solely because of the increased costs of salaries, wages, and materials. One gets a still smaller figure if one includes the fact that some of the new experimental techniques involve most expensive and elaborate equipment.

4. It is too early to judge how much increase in productivity the new funds will eventually bring about, because a substantial fraction of the money has been allocated for the construction of large accelerating machines, reactors, or other equipment most of which are not yet in operation.

5. The uncertain future of some of these sources of funds, the possibility but uncertainty of additional sources, the short term duration of the commitments, and, in some cases, the complex administrative, fiscal, or security arrangements have produced intangible effects difficult to evaluate.

These are the complicating factors, but the fact that more money is going into physics cannot be denied. It is going into higher salaries for physicists and into more money for equipment and for the stipends of fellows, assistants, technicians—and secretaries.

It is instructive to list the sources of these new funds:

1. Practically every university, private or state, has increased its physics department budget—and the presidents of these universities are now trying to find the funds which they promised.

2. Many industrial laboratories have expanded their basic research in physics along with a still greater expansion of their work in applied physics. Also many industries are now contributing research funds to universities.

3. A very large block of funds has been devoted to physics research by the Atomic Energy Commission in its laboratories at Oak Ridge, Argonne, Berkeley, Brookhaven, and Los Alamos. At some of these places, of course, as in industrial laboratories, the fields of activity are necessarily determined by their relevance to the main tasks of the Commission, namely, reactor or weapon development. However, the Atomic Energy Commission has also made its facilities available and has supplied materials to many university research groups and to an increasing extent is sponsoring university research projects.

4. Various agencies of the National Military Establishment have increased their support of pure and applied physics both in their own laboratories and by contracts with universities and industries. The Office of Naval Research alone now has university contracts in physics which involve expenditures at a current rate of over ten million dollars per year, a portion of which is covered by transfers from the Atomic Energy Commission.

5. And finally, one must not dismiss the GI bill which is financing the graduate work of hundreds of promising physicists, and a variety of other fellowship programs, including the new Atomic Energy Commission Fellowships. To those who too easily dismiss the value of fellowship programs, I should like only to recall that one of the most decisive factors in the great growth of basic science in this country in the 1920's and 1930's was the National Research Council Fellowship program financed by the Rockefeller Foundation. Without this great growth of science the war research program as we saw it could hardly have been imagined.

There are three obvious results of these new sources of funds which I wish to mention briefly.

1. The greatly increased demand for physicists. This I have already discussed. A new element in the picture is that physicists are now in greater demand for applied research and development work, funds for which have increased by a much larger factor than for pure

research. Thus the universities find themselves in greater competition than formerly not only with industrial laboratories but now also for the first time with a considerable group of large government laboratories. I refer not only to Atomic Energy Commission installations but to such laboratories as Naval Ordnance Laboratory, Naval Ordnance Test Station at Inyokern, Air Forces Laboratories at Wright Field, Watson Laboratories, the Signal Corps Laboratories, Naval Research Laboratories and others. Now, I am glad to see this increased demand for physicists by government laboratories. What we must be concerned about is only that pure research is not pushed out of the market for good men by lack of funds—a situation which we may indeed now be facing. On the other hand, it is reassuring to note that the universities still find it easier to fill their vacancies than do many applied research laboratories, in spite of a substantial salary differential.

2. A second result of the increased funds is, of course, the financing of a number of important large facilities, especially electronuclear machines, which no one, outside of Berkeley, could have thought about before the war. Those who feared that every physicist in the country was going to be wholly occupied in engineering new machines and doing no physics need only look at the current issues of the *Physical Review* and at the program of this meeting to have his fears allayed. By the end of another year these new instruments should be adding conspicuously to our store of new knowledge. Several are now in preliminary stages of operation. I have just come from a visit to the University of Rochester where the new cyclotron is producing 240 Mev protons and these are in turn producing mesons. The Berkeley synchroton is also now operating at full energy and gamma-ray produced mesons have been observed.

3. The third result of new funds has been a general stimulation of college and university research. This can be seen in a wide variety of instances. Any of my listeners can provide a dozen examples.

In discussing the effects of additional financial support of physics we must also mention its negative aspects. While those who predicted that physics would be wholly submerged by the flow of gold have been disappointed, it is still well to emphasize that gold is not everything. There are several grains of truth in Art Roberts' lilting and slightly irreverent song, "Take Away Your Billion Dollars."[1] Those who may be too young to remember the unclassified days of "love and string and sealing wax" may need to be reminded that not every significant experiment in physics requires a million dollar machine. Ideas and ingenuity are still worth more than many millions and we must not be tempted to believe otherwise.

And then there is the danger felt by many who note that much of the new support for physics is coming from those government agencies which are either wholly or partly devoted to military affairs. Now the regimentation of science, its subservience to immediate military ends, or undue secrecy restrictions would indeed be disastrous. Obviously, the disaster has not yet occurred. But there will be uneasiness until it is more clearly evident that the people of this country through *nonmilitary* agencies of their government and also as individuals and through corporations, foundations, and other agencies will insist on and will adequately support a strong and a free science. The spirit and significance of pure science and the meaning and necessity of strength and freedom are subjects on which we have a duty to speak—and on which we can also speak with competence.

Finally, there is the little recognized fact that the huge government appropriations of hundreds of millions of dollars for "research and development" are pretty largely allocated to applied research and to development. The amount that seeps through for basic research, especially in the universities, is a relatively small trickle indeed. There is still no Science Foundation or any other government agency devoted *primarily* to basic research of the sort to which the universities have been the primary contributors. Though the Office of Naval Research, aided recently by the Atomic Energy Commission, has nobly held the fort in this area for three years, its funds are now quite inadequate and will probably be subject to further substantial cuts. University research is not in such a lush position

[1] *Physics Today* 1, 17 (November, 1948).

as is generally assumed and its future is most insecure. We as a nation appear to be in the position of trying to erect a huge superstructure of applied science which must be built on a foundation of pure science, without first making sure that the foundation is sufficiently strong. Basic science is significantly better off than before the war as measured in dollars. But the heavy discount for inflation and the fact that funds are now decreasing rather than increasing suggests that the relative position of basic science has not been improved to nearly the extent to which its prestige has been enhanced, or to which the need for strengthening it in this country has grown.

There are those who argue that the funds now available for "research and development" are already far too large for the number of competent scientists available to spend them wisely. This may be true. Certainly there are many places where first rate budgets are being spent by second rate people. But I object again to the common tendency to bracket *research* with *development*. For when this is done it is development —or applied research—which draws most of the money and therewith attracts the men. I insist that mankind's future welfare would best be served by inverting the emphasis—by making sure that all competent scientists interested in basic research—pure research—are adequately supplied with necessary facilities *first*. Then one should adjust the budgets for *applied* research and development to the manpower resources available.

I could not complete a talk on the subject of physics in the postwar world without mentioning the worries which we all face, as citizens and as physicists, by virtue of the fact that World War II was followed not by peace but by what Dr. Conant calls an armed truce. The dark clouds of cold war overshadow every aspect of current living. Science in particular which thrives only in an atmosphere of mutual understanding and trust and which is nurtured by freedom of thought and of communication feels the dark clouds of suspicion and distrust most keenly. Science is frustrated in the face of iron curtains and armed guards. When, in a large share of the world, free science is buried under political blankets, or is struggling for its very existence against all but hopeless economic and social conditions the scientists of no country can be happy or fully effective.

And yet this is the kind of world in which we are destined to live for years to come. The issues which divide the world are not of the type which can be quickly settled. They involve basic differences on what is meant by such things as freedom and democracy.

We as physicists know that our science flourishes under conditions of complete freedom of research and of communication. We know that such conditions are far more nearly approximated in this country than in most other places in the world. We know that the restrictions under which we still labor are to a large extent inherent in the present world condition of cold war.

But we also believe that there are still restrictions being imposed upon the freedom of science and of scientists which are neither necessary nor desirable—which are indeed inimical not only to the welfare of science but also to the welfare of this nation.

The first of these restrictions is the set of secrecy rules—mistakenly referred to as security rules—with which science was necessarily enveloped during the war but which have not yet been adequately lifted. A major aspect of postwar physics between the years 1945 and 1949 has been this secrecy problem. We recognize that enormous progress has been made in opening up to publication the war-born scientific and technical developments. The 28-volume M.I.T. Radiation Laboratory Series contains practically all the essential technical developments which came about in the radar field. The forthcoming Manhattan District Series will contain most of the basic scientific work connected with that project. (The delay in preparation of this series incidentally is due as much to delays in getting the material written up as to difficulties with classification problems.)

But much remains to be done in reaching a properly balanced policy concerning how much secrecy is necessary or desirable for national security. Under present world conditions this problem is certain to be with us for some time to come. The demands of a few scientists that all secrecy restrictions be abolished is no nearer a reasonable solution than the demands of a few

military and political figures that everything be kept secret. The reasonable views recently expressed by K. T. Compton, new Chairman of the Research and Development Board (in his Ohio State University address reprinted in the December 1948 Bulletin of the *Atomic Scientists*) assures us that there is an understanding approach to the problem at the very highest level.

The so-called personal security or loyalty problem has been another feature of postwar science. Again Dr. Compton has stated a reasonable position on this matter which I need not repeat. In this area we as scientists will join with other citizens in insisting that there is no excuse for publishing unsupported charges of disloyalty against any American citizen—be he scientist, business man, or movie actor. We do not ask for any special privileges for scientists except insofar as their special position subjects them to a selective attack. We believe that the large contributions of scientists to the war effort, carried on, I believe, without a single important leak of information to the enemy, raises a presumption of unusual loyalty on the part of this group. We recognize the unfortunate necessity for personal security investigations of those engaged in classified work and believe these investigations must be not only effective but also fair. But irresponsible and unsupported public attacks are not in keeping with American principles of justice. We trust American science and American life will not be further threatened by fear of unjustified attacks against innocent men and women.

So there in broad outlines stands the picture of the science of physics in the postwar era. It suffers along with all other human activities from the shadows and uncertainties of a world which has been shattered physically and morally by a devastating war and which is not yet sure whether new and more terrible wars are yet to come. Physics also suffers, along with the other sciences, in this transition period in which the sources of financing of science are shifting from private funds to funds supplied by the states, by industry, or by the Federal government. It faces the danger that those who control these sources will be more interested in the immediate practical results than in long-term understanding of natural phenomena.

There are some who take a gloomy view of the future. At least one scientist has recently written, "The time is now well passed when scientists determine the path of the future of science. It is my belief," he says, "that the interdependent factors of research financing, world politics and legislative authority will be the greatest influences in determining the future of science. We scientists may fight the currents which appear unfavorable to us, but in the long run we will find ourselves ending up where the tides of economic and political forces carry us."

There is much logic in support of such a gloomy view of the future. But though logic may be our supreme instrument in arriving at valid scientific conclusions, the spirit of science itself involves something more than cold logic. The spirit of basic science, I believe, is stronger today than ever before. The men who make physics have been inspired by a new enthusiasm for their great science. They are profiting by many lessons learned during the war years. They have acquired new tools, they have learned new techniques, and they have become familiar with the values and limitations of cooperative efforts. If there are those in public life who misunderstand the spirit and significance of science and who will seek to divert it to false goals, there are on the other hand far more men than ever before in history, men in important positions, who do understand what science means and who will lend their support and encouragement to the development of a strong and a free science. And to this goal the scientists themselves will always remain dedicated.

And generally let every student of nature take this as a rule—that whatever his mind seizes and dwells upon with peculiar satisfaction is to be held in suspicion, and that so much the more care is to be taken in dealing with such questions to keep the understanding even and clear.—FRANCIS BACON, 1561–1626.

Can We Account for the Observed Abundances of the Chemical Elements?

D. TER HAAR
Purdue University, Lafayette, Indiana

DURING the last ten years many attempts have been made to give an explanation of the fact that the various chemical elements and their isotopes occur in different abundances. As far as can be judged from the necessarily scant data, the relative abundances of the various elements do not differ greatly from the earth to the sun or from one star to another. The only important difference between the earth and the sun is the high abundance of hydrogen and helium on the sun. This difference, however, can easily be understood from considerations about the origin of the earth.[1] In the present paper we shall attempt to sketch the present state of theories trying to account for the observed abundances. We shall not enter into a detailed discussion but refer for this to a forthcoming survey article.[2] The moment is the more propitious for presenting such an account since it seems that there exists at present a theory which can satisfactorily explain the various abundances, although it is not yet a completely quantitative theory as far as all details are concerned. However, a theory which would deal quantitatively with all details will probably not be forthcoming until our knowledge about stellar interiors and the influence of turbulence on the conditions in those interiors is much greater.

In the present paper we shall more or less follow the historical order of development. The complete picture will show how the various elements are formed in stellar interiors and are distributed over space by outbursts of these stars. It is, of course, possible to follow this process from the beginning down to the final stages. However, it is also quite possible that in this way important details might become obscured. For this reason, we shall proceed the other way, starting from a simple theory and building into that theory the necessary changes and details.

We have to show that all the chemical elements can actually have been formed in the interior of the stars. The first author to suggest that heavy elements might have been formed by nuclear reactions from hydrogen was Sterne.[3] His idea was put in a more or less quantitative form by von Weizsäcker[4] who suggested that the observed abundances should be the equilibrium concentrations of the various elements at some high temperature and density. Statistical thermodynamics can calculate the equilibrium concentrations of chemical compounds for a given temperature and pressure and exactly the same formulas can be used to calculate the equilibrium concentrations of the chemical elements. The role of the reaction heats in the chemical equilibria is taken over by the mass defects of the nuclei. Since the energies involved are much larger than in the case of chemical reactions, we should expect much higher temperatures and pressures. Von Weizsäcker's considerations and calculations were repeated by Chandrasekhar and Henrich[5] and by Klein, Beskow, and Treffenberg[6] using the much more accurate data about mass defects which became known after the publication of von Weizsäcker's paper. These three papers arrived at essentially the same conclusion and we shall therefore give the arguments of the Stockholm group[6] since we shall need their formulation for later discussions.

Klein, Beskow, and Treffenberg introduce a Gibbs grand ensemble.[7] The two constituents of this ensemble are protons with chemical potential μ and neutrons with chemical potential λ. If the mass defect of a nucleus is denoted by E, and if we neglect for the moment excited states of the nucleus, we have for the concentration of nuclei with N neutrons and Z protons

$$C(N, Z)=g(2\pi M\theta/h^2)^{\frac{3}{2}} \times\exp[(\lambda N+\mu Z-E)/\theta], \quad (1)$$

[1] D. ter Haar, *D. Kgl. Danske Vid. Selsk., Mat.-fys. Medd.* **25**, nr. 3 (1948).
[2] D. ter Haar, in course of publication.
[3] T. E. Sterne, *Monthly Notices* **93**, 726 (1933).
[4] C. F. von Weizsäcker, *Physikalische Zeits.* **38**, 176 (1937); *ibid* **39**, 633 (1938).
[5] S. Chandrasekhar and L. Henrich, *Astrophysical J.* **95**, 288 (1942).
[6] Klein, Beskow, and Treffenberg, *Arkiv f. Mat., Astron. och Fysik* **33B**, nr. 1 (1946).
[7] J. W. Gibbs, *Elementary principles in statistical mechanics*, Ch. XV; *Collected works* (Yale University Press, 1948), Vol. II.

where g is a weight factor, M the mass of the nucleus considered ($M = NM_n + ZM_p$), M_n the mass of a neutron, M_p the mass of a proton, and θ is the modulus of the ensemble which is connected with the temperature in the well-known way $\theta = kT$.

Klein, Beskow, and Treffenberg now choose the values of the three parameters θ, λ, and μ so as to obtain agreement between observed and calculated concentrations for the lightest elements. Having thus fixed the values of these parameters, they calculate from Eq. (1) the concentrations of the heavier elements. It then turns out that they obtain excellent agreement for nuclei with atomic weight up to about 70. For heavier elements, however, the calculated concentrations are far smaller than the observed ones; for the heaviest elements ($Z \sim 90$) a factor of the order of 10^{100} is missing!

It is interesting to contemplate the values of the temperatures and densities used by Klein, Beskow, and Treffenberg. They use

$$\theta = 1 \text{ Mev} \quad \text{or} \quad T = 10^{10} \text{ degrees}, \qquad (2)$$

and the total density is given by

$$\rho = \Sigma MC(N, Z) = 4 \times 10^{8} \text{ g cm}^{-3}. \qquad (3)$$

If we remind ourselves that the central temperature of the sun is only a few million degrees and the central density only a few g/cm^3, we see from Eqs. (2) and (3) that the temperature and density corresponding to the equilibrium, which should have provided the chemical elements, are indeed very high.

We must remark here that, of course, Klein, Beskow, and Treffenberg, and the other authors mentioned, assume that the transition from these high temperatures and densities to normal conditions takes place so rapidly that the equilibrium concentrations are not changed appreciably during the process: the equilibrium is *frozen*.

Since it is impossible to account in this way for the relatively high concentrations of heavy elements, it is necessary to search for effects which may be important and which may have been overlooked in the rather simplified picture given in the preceding paragraphs, provided, of course, that we really wish to account for all the elements in the way indicated at the beginning of this paper. We should especially look out for those effects which may be neglected under normal circumstances but become important at high temperatures or high densities. The first effect (*1*) is the influence of excited nuclear states. The spacing of the nuclear levels is, for high temperatures, no longer large compared to kT, and higher energy levels play a role. The second effect (*2*) is the influence of electrostatic forces. At the high densities which we are considering the mean distances between the nuclei and between nuclei and electrons will become so small that the electrostatic energy might play a part. The third effect (*3*) is the effect of gravitational fields on the equilibrium. Since we are considering stars with great central densities, we must expect that gravitation has possibly played a role too.

Apart from these effects, we shall have to investigate the following questions: (*4*) Is it reasonable to assume that the transition from high temperatures and densities to normal conditions can take place so rapidly that the equilibrium is frozen in? (*5*) What will be the effect of radioactive transitions from nuclei formed at high temperatures to the nuclei observed at present? (*6*) Is it possible to find a place in space where the processes which we are going to consider can take place? (*7*) Could it be that the processes leading to the chemical elements are connected with the processes leading to the formation of stars and galaxies? In other words: is there a place for the processes leading to the formation of the chemical elements in a general cosmogony? In the following, we shall discuss these points separately.

(*1*). *Excited Nuclear States.*—The influence of excited states on the concentrations of the various nuclei was independently investigated by Unsöld,[8] Géhéniau, Prigogine and Demeur[9] and Beskow and Treffenberg.[10] We shall here sketch the method of Beskow and Treffenberg. They take the excited states into account by taking for the weight factor g the product of the partition functions pertaining to the rotational and vibrational levels of the nucleus. The rotational partition function can easily be evaluated and varies as $A^{5/2}$, if A is the atomic weight of the nucleus.[8] The evaluation of the vibrational partition func-

[8] A. Unsöld, *Zeits. f. Astrophysik* **24**, 278 (1948).
[9] Géhéniau, Prigogine, and Demeur, *Physica* **13**, 429 (1947).
[10] G. Beskow and L. Treffenberg, *Arkiv f. Mat., Astron. och. Fys.* **34A**, nr. 13 (1947).

TABLE I. Values of nuclear charge, atomic weight and specific charge at various densities.

ρ (g cm^{-3})	5×10^{9}	4×10^{10}	10^{11}	2×10^{11}
A	60	100	140	180
Z	27	37	45	52
Z_A	27	43	59	73

tion is slightly more complicated. It involves an estimate of the distribution of the vibrational levels in the energy spectrum. For this distribution, use can be made of the expressions given by Bohr and Kalckar[11] or van Lier and Uhlenbeck.[12] The vibrational partition function also increases with increasing atomic weight.

Beskow and Treffenberg finally find that, although the excited states give rise to a factor 10^{20} for uranium, the effect is not large enough to account for the observed abundances of the heavy elements.

(*2*). *Electrostatic Effects.*—Van Albada[13] has drawn particular attention to two effects which start to play a role only at high densities. In order to simplify the considerations, he considers systems at zero temperature so that the only type of nucleus present will be that for which the total energy per unit mass is minimum.

Let us consider for a moment a system consisting of nuclei of charge Z and atomic weight A. For low densities, the energy per unit mass will be determined by the mass defect of this particular nucleus. We shall denote by Z_A the charge for which the mass defect is minimum for a given value of A. For low densities, we can safely neglect the energy due to the Z electrons which are present for every nucleus of charge Z (we assume the whole system to be electrically neutral). This energy consists of two parts, the Coulomb energy, and the zero-point energy of the electron-gas which is a quantum gas with a large Fermi-Dirac zero-point energy. Both these energies increase with increasing density and both are negligible at normal densities as compared to the mass defect. However, the mass defect does not depend on the density, whereas these energies due to the electrons depend strongly on the density. For densities of the order of those which we met before in Eq. (3) they already play an important role. The Coulomb energy will lower the specific charge (Z/A) of the nuclei, since nuclei with low specific charge will contribute less to this energy. The zero-point energy favors nuclei of high atomic weight, as was shown by van Albada. Using van Albada's formulas, one can calculate for which values of Z and A the energy content per unit mass is minimum. In Table I we have assembled these values of A and Z for various values of the density ρ. In the last row of the table we have inserted the values of Z_A corresponding to the A-values of the second row.

Table I shows without doubt that high densities favor the formation of nuclei with high Z and A/Z values. The nuclei coming from an equilibrium at such high temperatures must therefore be expected to contain more neutrons than the nuclei found in nature.

Although the effects considered here have a tendency to shift the equilibrium towards higher atomic weights, it is not possible to obtain nuclei of, say, atomic weight 250 since van Albada has shown that for densities larger than the largest in Table I, it will be energetically more favorable to form free neutrons than to form heavy nuclei. We have thus to look for still another effect in order to get the heaviest nuclei of the periodic system.

It is, of course, also clear that van Albada's theory is only the first step in making the necessary calculations since we have to take into account the fact that for actual temperatures, which are not zero, other nuclei besides the ones with the lowest energy per unit mass will be present.

(*3*). *Gravitational Effects.*—Gibbs[14] has indicated how we can take into account the influence of a gravitational or electric field on a system in thermodynamic equilibrium. If we make use of the conclusion arrived at below in Section (*6*) that the elements are probably formed in the interior of stars, we can try to consider the influence of the gravitational field of such a star. Beskow and Treffenberg[15] have used Gibbs'

[11] N. Bohr and F. Kalckar, *D. Kgl. Danske Vid. Selsk., Mat.-fys. Medd.* **14**, nr. 10 (1937).

[12] C. van Lier and G. E. Uhlenbeck, *Physica* **4**, 531 (1937).

[13] G. B. van Albada, *Bulletin of the Astronomical Institutes of the Netherlands* **10**, 161 (1946); *Astrophysical J.* **105**, 393 (1947).

[14] J. W. Gibbs, *Transactions Connecticut Academy* **3**, 108 (1875); also, *Collected works* (Yale University Press, 1948), Vol. I, p. 144.

[15] G. Beskow and L. Treffenberg, *Arkiv f. Mat., Astron. och Fys.* **34A**, nr. 17 (1947).

method for these calculations. We shall try to sketch their considerations briefly.

Consider a system under the influence of a gravitational potential φ_g and an electric potential φ_e. For equilibrium, the following equations have to be satisfied for any value of Z and N:[16]

$$(Z\mu+N\lambda)+(ZM_p+NM_n)\varphi_g+Ze\varphi_e=\text{const.}, \quad (4)$$

where μ, λ, φ_g and φ_e are still functions of the space coordinates. Equation (4) will always be satisfied, if it is satisfied for the neutron ($N=1$, $Z=0$) and the proton ($N=0$, $Z=1$). This means that we have to satisfy the two equations:

$$\lambda+M_n\varphi_g=\text{const.}, \quad (5)$$

and

$$\mu+M_p\varphi_g+e\varphi_e=\text{const.} \quad (6)$$

For the potentials φ_g and φ_e, we can write down the usual Poisson equations, which, in the case of φ_g, for instance, has the form

$$\nabla^2\varphi_g=4\pi G\rho, \quad (7)$$

where G is the gravitational constant and ρ is the total density given by

$$\rho=\Sigma C(N,Z)(NM_n+ZM_p). \quad (8)$$

Eliminating φ_g from Eqs. (5) and (7) and eliminating φ_e in a similar way, we are left with two equations giving relations between λ, μ, and ρ. Since the $C(N, Z)$ can be expressed in terms of λ and μ by using Eq. (1), we see that Eq. (8) gives us the third equation involving λ, μ, and ρ so that we can solve for λ, μ, and ρ as functions of the position in the star. From Eq. (1) we can then find at any position in the star the concentrations of the various nuclei. For all these calculations, Beskow and Treffenberg used the θ-value given by Eq. (2). Integrating over the whole star, it is possible to get the total relative abundances. Since these results do not vary greatly from one stellar model to another, we have given in Table II the results for one of the models investigated by Beskow and Treffenberg. In the first row we give the logarithms, to the base 10, of the concentrations; while in the second row we give the logarithms of the observed concentrations as quoted by Goldschmidt.[17] For details regarding the calculations leading to the values given in Table II, we refer to Beskow and Treffenberg's paper.[15] We see now that the calculated concentrations of even the heaviest elements are of the same order of magnitude as the observed ones.

TABLE II. Relative concentrations of various nuclei.

Atom	H	He	C	Fe	Sn	U	A = 300
$\log(C/C_H)_{calc}$	0	−1.9	−10.2	−8.7	−9.1	−9.5	−9.2
$\log(C/C_H)_{obs}$	0	−1.4	−2.0	−3.4	−8.4	−10.0	—

(4). *Transition to Normal Temperatures and Densities.*—The weakest point in the theory which we have just sketched is the question whether the transition from high temperatures and densities to normal conditions can take place so fast that the equilibrium concentrations are frozen in. The mechanism through which the elements are distributed over space should be the sudden breakup of the star, in the interior of which the elements are formed. It is very tempting to identify this breakup with a supernova outburst. Both Hoyle[18] and van Albada[13] make this tentative identification. It is necessary to look for such a sudden outburst since otherwise the equilibrium will change slowly and the final concentrations will no longer correspond to the equilibrium concentrations calculated in the preceding paragraph.

As yet there is no theoretical estimate of the velocity of a supernova outburst, although Hoyle optimistically estimates the total outburst to last only a few seconds. Since the equilibrium also needs a few seconds to be established at high temperatures and densities,[18] the freezing down of the original equilibrium situation should be almost complete. Van Albada is much more conservative in his estimate of the velocity of the supernova outburst. In favor of the assumption that the whole process is extremely rapid is the observational result that a supernova attains maximum brightness in at most a few days. Since

[16] It may be remarked here that by introducing the electric potentials, the electrostatic effects discussed in Section 2 are taken into account automatically. It is also possible to take the Fermi-Dirac zero-point energy into account, and this is indeed done by Beskow and Treffenberg (see reference 15). A discussion of how this can be done falls, however, outside the scope of this paper.

[17] V. M. Goldschmidt, *D. Norske Vid. Akad. i Oslo, Mat. Naturv, Kl.* nr. 4 (1937).

[18] F. Hoyle, *Monthly Notices* **106**, 343 (1947).

the real maximum, accompanied by a large amount of emitted ultraviolet radiation which cannot be observed, must have been attained much earlier, we get an upper limit for the velocity with which the elements are distributed over space.

Although this point ought to be investigated in much more detail (an extremely difficult and at present probably impossible theoretical problem), it seems that as yet there is no reason to assume that the theory would break down here. We may add that Hoyle[18] assumes that the supernova outburst occurs when a star, which has used up all its hydrogen, becomes rotationally unstable (see Sec. (*6*)).

(*5*). *Transition to Stable Nuclei.*—We have seen that the nuclei formed in stellar interiors will possess an excess of neutrons. Under normal conditions these nuclei will not be stable but will show fission. The fission products will again be unstable and through radioactive changes decay into stable nuclei. These processes have been considered in detail by Mayer and Teller,[19] who assume that after the fission of the neutron-rich nuclei, the residual nuclei will be highly excited. This excess energy will, first of all, result in a loss of neutrons through "evaporation from the droplet of nuclear fluid." If the energy of the nucleus has decreased so far that no more neutrons can evaporate, normal radioactive processes, of which β-processes are the most frequent, will follow. The final result will be a stable nucleus—one of the isotopes observed in nature. Starting from the concentrations of neutron-rich nuclei from the stellar interior, and making reasonable assumptions as to the excitation energy of the fission products, one can calculate the abundances of the stable isotopes. Mayer and Teller found for these abundances good agreement between observed and calculated values.

Combining the results discussed so far, we see that an equilibrium theory seems to be able to account quantitatively for the observed abundances of stable isotopes. It is immediately clear that this theory is as yet only a rather rough outline and detailed analyses are necessary to back up the preliminary results which are, however, very encouraging.

(*6*). *Where Are the Nuclei Formed?*—The next question which now comes up is whether there exist in the universe places where the above mentioned exceedingly high temperatures and densities can be found. There are probably many people who would prefer a theory in which all the nuclei of the universe were formed together at the beginning of time. However, from general relativity it follows immediately that if all the matter in the universe were together in one system with a density of only 10^6 g cm^{-3} this system would explode in less than a second. It is, therefore, impossible to put all the matter of the universe together into one very dense system and we have to look for smaller conglomerations of matter. We are then immediately led to an investigation whether there are stars, the central temperatures and densities of which are sufficiently high. These stars can indeed be found.

Hoyle[18] has drawn attention to the fact that the highly luminous O-, B-, and A-stars are burning up their hydrogen so rapidly that during the lifetime of the universe, which is between 10^9 and 10^{10} years,[2, 8] many of these stars must use up all their hydrogen. When the hydrogen is used up, the star will start to contract in order to supply energy from its gravitational potential instead of from the carbon-nitrogen cycle. This contraction cannot, however, go on indefinitely, if the star possesses an angular momentum. Due to the conservation of angular momentum, the rotational velocity will increase during the contraction until the centrifugal forces will have become of the same order of magnitude as the gravitational forces. At that moment, an explosion will result, which according to Hoyle will be of a violent nature. Hoyle has also shown that some stars will attain central temperatures and densities sufficiently high to produce heavy nuclei before exploding.

In this way, we have found places where the heavy elements can be formed. The next question is, whether there is a sufficient number of supernova outbursts to account for the total amount of heavy elements, assuming that originally no heavy elements were present. This question and the relation between the origin of the elements and general cosmogony will be taken up in the next section.

(*7*). *Is There a Place for the Origin of the*

[19] M. G. Mayer and E. Teller, Solvay Congress, 1948.

Elements in General Cosmogony?—If one accepts von Weizsäcker's general cosmogony,[20] it can easily be seen that the processes discussed above fit in well with his picture of the development of the universe.[21] Von Weizsäcker assumes that all stars and galaxies are formed from an initially turbulent gaseous system. In what follows, we shall assume that this gas consisted of hydrogen only. Since the final result will be the present distribution of the elements, it is clear that the condition that only hydrogen was present initially is immaterial. Due to the viscosity of the gas, the turbulent system will lose energy and the turbulence elements will contract. The final stages of the turbulence elements will be stars (we do not wish to discuss here the intermediate steps leading to galaxies, etc.). The energy of these stars will first be produced by contraction,—a gravitational source of energy. The next stage will be when the deuterium reaction ($H+H \rightarrow D$) supplies the energy. During that stage some other elements will also be formed, such as carbon, nitrogen and oxygen. After the deuterium stage, the carbon-nitrogen cycle will set in and the star will belong to the early type main-sequence stars.[22] We have now got the O-, B-, and A-stars and the subsequent development may well be along the lines discussed in the preceding sections.

If we now try to estimate how large an amount of heavy elements the O-, B-, and A-stars can account for, we have to take the percentage of mass taken up by these luminous stars, and multiply this by the fraction of these stars which may attain a sufficiently high central density. Using Kuiper's data[23] it then turns out that there is as yet no argument against this process having provided us with the heavy elements in their present abundances.[21]

(*8*). *The α-β-γ-Theory.* Recently, Gamow and Alpher[24] have investigated the possibility of accounting for all the nuclei of the universe in one big reaction. The idea was that a few hours after the universe started expanding, nuclei should be built up from the original neutron gas. The neutrons in this gas should form protons by β-decay and subsequent neutron captures and β-decays should account for the other elements. Alpher's calculations seem to give good agreement between observed and calculated abundances.

It seems to us, however, that there are many objections which can be raised against this theory. Alpher[24] has himself listed a number of these objections. Another point which can be raised against this theory is the following. In the preceding sections, especially in Sec. (*7*), we saw that the present distribution of the chemical elements is to a large extent independent of the distribution assumed to exist previous to the formation of the stars. If this is really true, the Alpher-Gamow theory would be irrelevant to the present distribution. The fact that the Alpher-Gamow theory predicts about the same abundances as the equilibrium theory would then indicate that the present situation is more or less stationary.

[20] C. F. von Weizsäcker, *Zeits. f. Astrophysik* **24**, 181 (1947).

[21] D. ter Haar, *Science* **109**, 81 (1949).

[22] We are not discussing or attempting to discuss here the origin of the stars outside the main sequence. The problems connected with their origin are extremely difficult to tackle.

[23] G. P. Kuiper, *Astron. J.* **53**, 194 (1948).

[24] Alpher, Bethe, Gamow, *Physical Rev.* **73**, 803 (1948); G. Gamow, *Nature* **162**, 680 (1948); Alpher, Herman, and Gamow, *Physical Rev.* **74**, 1198 (1948); R. A. Alpher and R. Herman, *Nature* **162**, 774 (1948); R. A. Alpher, *Physical Rev.* **74**, 1577 (1948); R. A. Alpher and R. Herman, *Physical Rev.* **74**, 1737 (1948); J. S. Smart, *Physical Rev.* **74**, 1882 (1948).

Thou shalt take three mirrors, and set two of them at an equal distance from thee, and let the other, more remote, meet thine eyes between the first two. Turning toward them, cause a light to be placed behind thy back, which may shine upon the three mirrors, and return to thee reflected from all. Although the more distant image may not reach thee so great in quantity, thou wilt there see how it must needs be of equal brightness with the others.—DANTE, *The Divine Comedy*, Canto II. Translated by CHARLES ELIOT NORTON.

Microwave Methods in Physics
I. Microwave Spectroscopy

C. Kikuchi and R. D. Spence
Michigan State College, East Lansing, Michigan

IN the past, the phrase "optical precision" has been considered descriptive of the ultimate accuracy that could be attained by physical measurements. It has served as a high standard of excellence that other methods of physical measurements might hope to equal but never excel. Recently a new branch of physics called microwave physics has developed, which can claim a comparable degree of precision. Because frequencies can be measured with a high degree of precision in the microwave region, and because the energy associated with a microwave photon is small, it has become possible to measure certain molecular, atomic, and nuclear constants to a higher degree of precision than was possible heretofore. As a result, theoretical speculations which were previously considered purely abstruse mathematical calculations have been brought within the range of experimental verification. In some fields of physics microwaves are such powerful tools that whole problems are being investigated anew; in others experimental evidence has already been obtained that permits modification and extension of theories.

Stimulated by these interesting developments, we have undertaken a survey of the fields in which microwaves have found applications. The series of papers we are preparing however, is not intended for those who are at the moment actively engaged in research in this field. Our primary purpose is to acquaint others with the underlying principles, the fundamental concepts, and the scope of applicability of microwaves. Thus, we have chosen to discuss a few representative, but varied, fields of investigations to which microwaves have been applied successfully. In carrying out this purpose, we have consciously sacrificed completeness in matters of detail for boldness of treatment, believing that the reader who wishes to follow up particular aspects of the subject will avail himself of the references at the ends of the individual papers.

The microwave spectrum is generally considered as lying between the wavelengths of 0.25 and 15 cm. Such limits are obviously rather arbitrary and are justified only by the fact that in this range the physical problems of generation, transmission, and detection of electromagnetic energy are sufficiently similar to be solved by common techniques. While microwave techniques could certainly be employed at longer wavelengths, the wave guides, cavity resonators, and other apparatus which characterize microwave methods are generally considered too bulky to be convenient. On the other hand, the high frequency limit of the spectrum appears to be fixed by our lack of ability to make the same types of apparatus very small. At present it is not quite clear where this upper limit is, and it may well be that in the future the present microwave methods will enable us to reach even higher frequencies. However, it seems quite certain that the gap between the far infra-red and the microwave spectrum will not be completely bridged without drastic modification of present day techniques. This no man's land of the electromagnetic spectrum offers a great challenge for future work.

The present apparatus of microwave physics is almost entirely inherited from wartime radar. Very little, if any, entirely new apparatus for microwave research has appeared since the war. The reasons for this apparently arrested development lie in the systematic thoroughness with which the scientists working on radar exploited the microwave field, and the ease with which components of radar systems can be fitted together to form satisfactory research setups. A research system constructed from radar components may well be capable of high precision but is apt to be limited in its flexibility as is the case with the parent materials.

The list of applications of microwaves in physics is growing rapidly. Some of the applications lie in the study of heavy molecules, para- and ferro-magnetic substances, dielectrics, structure of absorption lines, and skin effect at low temperatures. In the series of reports we have

planned, we shall consider these applications in turn, beginning with microwave spectroscopy.

I. Microwave Spectroscopy

Microwave spectroscopy had its origin in the pioneer work of Cleeton and Williams on the inversion spectrum of ammonia.[1] Despite the fact that this work was reported in 1934, it was not until after the modern microwave techniques of wartime radar appeared in the research laboratory that the field of microwave spectroscopy became popular. Since it is not possible to review here the large amount of work which has been done in the field, we will limit our discussion to the general principles and scope of microwave techniques as illustrated by the work on certain polyatomic molecules.

Emission spectra at wavelengths in the microwave range (0.25–15 cm) are completely masked by temperature radiation from nearby objects and therefore microwave spectroscopy deals entirely with absorption spectra. As in other regions of the electromagnetic spectrum, electromagnetic energy of frequency ν will have a high probability of being absorbed only if it is possible for the absorbing molecules or atoms to make transitions between two states whose energies E_1 and E_2 satisfy the relation

$$h\nu=(E_2-E_1), \tag{1}$$

where h is Planck's constant[2] (6.55×10^{-27} erg sec). For example, if absorption takes place in the middle of the visible spectrum ($\sim$5000A) the energy of the two states must differ by about 2.5 ev. However, if absorption takes place in the microwave region at a wavelength of 5 cm, the energy separation need only be 1/100,000 of the previous value, or 2.5×10^{-5} ev. Since each such transition involves the absorption of a rather small amount of energy, there must be a large number of such transitions per unit time in order that an observable amount of power be absorbed. This means that a large number of the molecules or atoms in the absorbing material must be in the lower energy state E_1 and the probability of transition from the lower state E_1 to upper state

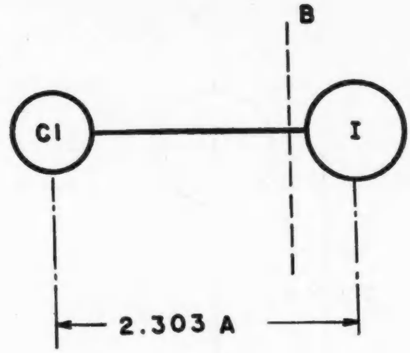

FIG. 1. The linear molecule ICl. The moment of inertia I_B is referred to the axis B through the center of mass.

E_2 must be large. It appears that if the difference E_2-E_1 is such as to correspond to absorption in the microwave range, the above conditions can be satisfied by fairly heavy molecules making pure rotational transitions.

One may obtain approximate expressions for the rotational energy levels of a molecule by considering it as a rigid body whose rotational motion is subject to quantum-mechanical rules. If one considers a linear molecule such as the molecule of ICl shown in Fig. 1, calculation shows that the energy levels are given by[3]

$$E_r(J)=\frac{h^2}{8\pi^2I_B}J(J+1), \tag{2}$$

where I_B is the moment of inertia computed about the axis through the center of mass. For symmetrical top molecules, such as CH_3I shown in Fig. 2, the energy levels are given by[4]

$$E_r(J)=\frac{h^2}{8\pi^2I_B}J(J+1)+\frac{h^2}{8\pi^2}\left(\frac{1}{I_A}-\frac{1}{I_B'}\right)K. \tag{3}$$

Here I_A is the moment of inertia about the figure axis and I_B is the moment of inertia about the axis perpendicular to the figure axis and through the center of mass (see Fig. 2). In both Eqs. (2) and (3), J is the rotational quantum number and may be considered as the magnitude of the vector J which represents the total angular momentum due to molecular rotation in units of $h/2\pi$; and K

[1] C. E. Cleeton and N. H. Williams, *Physical Rev.* **45**, 234 (1934).

[2] The numerical values of the natural constants are taken from the paper by J. W. M. DuMond and E. R. Cohen, *Rev. Mod. Physics* **20**, 82 (1948).

[3] L. Pauling and E. B. Wilson, *Introduction to quantum mechanics* (McGraw-Hill, New York, 1935), p. 271.

[4] See reference 3, p. 275.

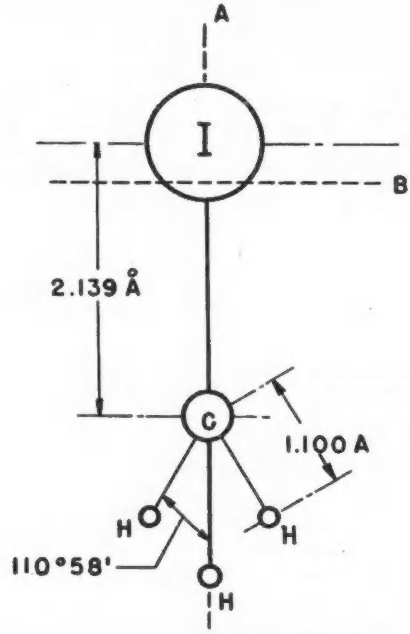

FIG. 2. The symmetric top molecule CH_3I. The moments of inertia I_A and I_B are referred to axes A and B respectively. The angles between the C−H bonds are equal and form a symmetrical tetrahedron. The data for this figure are taken from the paper of W. Gordy *et al.* (reference 9).

represents the projection of J on the figure axis of the molecule. The energy absorbed by either a linear or symmetric top molecule in transition from the state J to $J+1(\Delta K=0$ for symmetric top molecules) is given by

$$E_r(J+1)-E(J)=\frac{h^2}{8\pi^2 I_B}2(J+1). \tag{4}$$

The corresponding frequency is

$$\nu=\frac{h}{8\pi^2 I_B}2(J+1), \tag{5}$$

or expressed as a wavelength,

$$\lambda=\frac{8\pi^2 c I_B}{h2(J+1)}=\frac{I_B}{27.99\{2(J+1)\}}\times 10^{40}\text{ cm}. \tag{6}$$

If the molecules are considered as two point masses m_1 and m_2 separated by a distance a, the moment of inertia about the center of mass in g cm² is

$$I_B=\frac{m_1 m_2}{m_1+m_2}a^2, \tag{7}$$

or expressed in terms of the respective molecular weights M_1 and M_2

$$I_B=\frac{1}{6.02\times 10^{23}}\frac{M_1 M_2}{M_1+M_2}a^2. \tag{8}$$

As shown in Table I, many molecules of the type we are considering have an internuclear distance a in the range between slightly greater than 1A to slightly greater than 2A. Thus one finds that I_B is of the order of $M\times 10^{-40}$ g cm², where M is the reduced molecular weight $M_1M_2/(M_1+M_2)$. Thus for the molecule ICl, $a=2.30A$, and

$$I_B=\frac{1}{6.02\times 10^{23}}\frac{127\times 35.5}{127+35.5}\times(2.30\times 10^{-8})^2$$
$$=240\times 10^{-40}\text{ g cm}^2. \tag{9}$$

In the light of these considerations it appears that if the wavelength of the incident radiation is of the order of

$$\lambda \sim M/100\text{ cm}, \tag{10}$$

where M is the reduced molecular weight of the gas, one can expect absorption arising from transitions from the energy level $J=1$. Thus it appears that a gas whose reduced molecular weight M is the order of 40 or greater is required to produce absorption lines in the microwave range ($\lambda>0.5$ cm).

One spectrum which has been extensively investigated by microwave methods and which cannot be classed as rotational is the inversion spectrum of ammonia. This will be discussed in greater detail in a later paper.

In the previous discussion, the nuclei have been considered as point charges or else spherical with a uniform charge distribution. This may be an unwarranted over-simplification if one is able to measure very small energy differences of the magnitudes associated with microwave quanta. A simple way to take this into account is to suppose that the nuclei are no longer spherical, but that the nuclear charge is still uniformly distributed throughout the volume of the nucleus. The shift in the energy levels of the molecule which arises from the nonspherical shape of the

Table I. Data on molecular and nuclear structure derived from microwave spectroscopy.

Molecule composition: Chemical	Isotopic	J transition	B_0* Mc/sec	$I_B\times10^{40}$ g cm^2	$I_A\times10^{40}$ g cm^2	Internuclear distance (A)	Quadrupole coupling constant Mc/sec**	Ref.***
BrCN	$Br^{79}C^{12}N^{14}$	2→3	4129.19	203.6	—		Br, 686.5	*l*
		3→4	4120.22	203.6	—	C−B$_2$, 1.79	Br, 686	*b*
	$Br^{79}C^{13}N^{14}$	3→4	4073.36	206.0	—		Br, 686	*b*
	$Br^{81}C^{12}N^{14}$	2→3	4096.76	204.8	—	C−N, 1.16	Br, 573.5	*l*
		3→4	4096.80	204.8	—		Br, 573	*b*
	$Br^{81}C^{13}N^{14}$	3→4	4049.61	207.2	—		Br, 573	*b*
ICN	$I^{127}C^{12}N^{14}$	3→4	3225.53	260.1	—		I, −2420	*l*
	$I^{127}C^{13}N^{14}$	4→5	3225.56	260.1	—	I−C, 1.99	I, −2420	*b*
	$I^{127}C^{13}N^{14}$	4→5	3177.04	264.1	—	C−N, 1.16	I, −2420	*b*
OCS	$O^{16}C^{12}S^{32}$	1→2	6081.48	137.2				*l*
	$O^{16}C^{12}S^{33}$	1→2	6004.92	139.7		C−O, 1.16	C^{13}, 0.5	*l*
	$O^{16}C^{12}S^{34}$	1→2	5932.83	141.4			S^{34}, 1.0	*l*
	$O^{16}C^{13}S^{32}$	1→2	6061.92	138.4		C−S, 1.56		*l*
	$O^{16}C^{13}S^{34}$	1→2	5911.73	141.9			O^{18} 1.0	*l*
	$O^{16}C^{13}S^{34}$	1→2	6043.25	138.8				*l*
	$O^{18}C^{12}S^{32}$	1→2	5704.83	147.1				*l*
CH_3Cl	$C^{12}H_3Cl^{35}$	0→1	13,292.89	63.11	5.52	C−Cl, 1.779	−75.13	*a*
	$C^{12}H_3Cl^{37}$	0→1	13,088.19	64.10	5.52	C−H, 1.109	−59.03	*a*
CH_3Br	$C^{12}H_3Br^{79}$	1→2	9568.10	87.68	5.50	C−Br, 1.936	577.0	*a*
	$C^{12}H_3Br^{81}$	1→2	9531.74	88.01	5.50	C−H, 1.104	482.0	*a*
CH_3I	$C^{12}H_3I^{127}$	1→2	9501.40	111.8	5.50	C−I, 2.139		*a*
	$C^{13}H_3I^{127}$	1→2	7119.04	117.8	5.50	C−H, 1.100	−1934	*a*
ClCH	$Cl^{35}C^{12}N^{14}$	1→2	5970.82	140.5			{Cl, −83.2 {N^{14}, −5.63	*l*
	$Cl^{35}C^{12}N^{14}$	2→3	5970.82	140.5		Cl−C, 1.63	Cl, −83.5	*b*
	$Cl^{35}C^{13}N^{14}$	2→3	5939.80	141.2			Cl, −83.5	*b*
	$Cl^{37}C^{12}N^{14}$	1→2	5847.26	143.5		C−N, 1.17	{Cl, −65.7 {N^{14} −3.63	*l*
	$Cl^{37}C^{12}N^{14}$	2→3	5847.24	143.5			Cl, −65.0	*b*
	$Cl^{37}C^{13}N^{14}$	2→3	5814.71	144.3			Cl, −65.0	*b*
AsF_3	$As^{75}F_3$	1→2	5879	142.7		As−F, 1.71	−235	*c*
N_2O	$N^{14}N^{14}O^{16}$	0→1	12,137.3	66.79		N−N, 1.13		*g*
	$N^{15}N^{14}O^{16}$	0→1	12,561.6	69.12		N−O, 1.19		
HNCS	$HNC^{12}S^{32}$	1→2	5866	143.0				
	$DNC^{12}S^{32}$	1→2	5474	153.3		H−N, 1.2		*h*
	$HNC^{13}S^{32}$	1→2	5847	143.5		N−C, 1.21		
	$DNC^{13}S^{32}$	1→2	5460	153.7		CS, 1.57		
	$HNC^{12}S^{34}$	1→2	5729	146.5				
ICl	$I^{127}Cl^{35}$	0→1	3498	239.7				*i*
		3→4	3422.3	245.2		2.30	I, −2930.0	*j*
	$I^{127}Cl^{37}$	0→1	3352	250.2				*i*

* $B_0=\nu_0/2(J+1)$, where ν_0 is the rotational frequency in the absence of the quadrupole interaction, and J is the quantum number of the lower state. Note that B_0 is given in mc/sec.

** The quadrupole coupling constant is $(1/h)eQ(\partial^2V/\partial Z^2)\times10^{-6}$ mc/sec.

*** These references appear at the end of Table II.

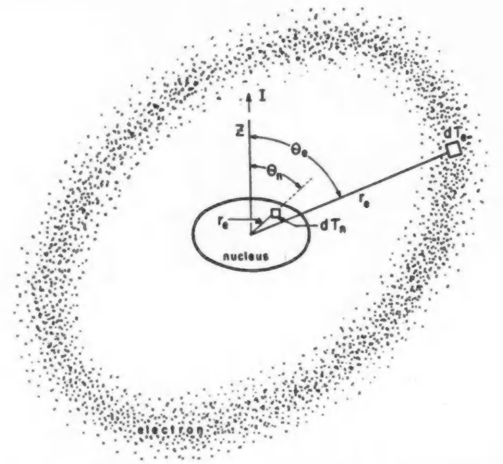

FIG. 3. The coordinate system used in calculating the quadrupole interaction. The volume element $d\tau_n$ lies in the nucleus whose flattened shape is considered to be responsible for the nuclear quadrupole moment. The volume element $d\tau_e$ lies in the electron charge distribution surrounding the nucleus.

nucleus may be estimated by use of classical electrostatics if one makes the simplifying assumptions that the nucleus has axial and equatorial symmetry. Such assumptions may be made plausible by considering the rapid rotation of the nucleus about its axis. The electrostatic energy of interaction may then be expressed as a series of inverse powers of the distance between elements of charge in the nucleus and the electron. The first term is the so-called Coulomb or $1/r$ term, representing the interaction energy which would exist if the charge of the nucleus were concentrated at its center. The next most important term is called the electric quadrupole term which may be written[5]

$$E_Q=(eQ/4)\int\left(\frac{\rho_e}{r_e^3}\right)(3\cos^2\theta_e-1)d\tau_e, \quad (11)$$

where

$$eQ\equiv\int\rho_n r_n^2(3\cos^2\theta_n-1)d\tau_n. \quad (12)$$

The integrations are to be carried out over the distributed electron and nuclear charge whose densities are ρ_e and ρ_n, respectively. The meaning of the variables in these expressions is shown in Fig. 3. The quantity Q is called the nuclear quadrupole moment. It is a measure of the deviation of the nuclear charge from spherical symmetry; it may be either positive or negative, depending on whether the nucleus is elongated in the direction of the nuclear axis of rotation or flattened in a plane perpendicular to it (see Fig. 4). Since the maximum value of $(3\cos^2\theta-1)$ is 2, and since the product $\rho_n r_n^2$ is always positive,

$$|eQ|<2\int\rho_n r_n^2 d\tau_n. \quad (13)$$

Hence

$$|eQ|<2r^2_{n(\max)}\int\rho_n d\tau=2Zer^2_{n(\max)}, \quad (14)$$

where Z is the atomic number of the nucleus and $e=4.8\times10^{-10}$ esu. The maximum radial dimension r_n of the nucleus is of the order of 10^{-12} cm. Hence

$$|eQ|<Z\times10^{-33}\text{ esu cm}^2. \quad (15)$$

In a similar fashion

$$|E_Q|<(|Q|/2)(e/r^3_{e(\min)}). \quad (16)$$

If the distance $r_{e(\min)}$ between the electron and the nucleus is taken as 1A then

$$|E_Q|<20Z\times10^{-20}\text{ ergs}. \quad (17)$$

which corresponds to a frequency splitting

$$\Delta\nu<30Z\text{ megacycles/sec}. \quad (18)$$

Equation (18) gives only an upper bound and merely indicates that the energy involved in the quadrupole interaction might be observable in

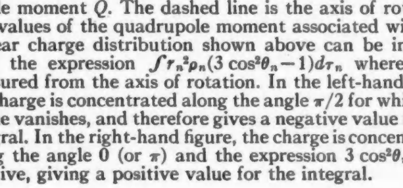

FIG. 4. The effect of shape of the nucleus on the quadrupole moment Q. The dashed line is the axis of rotation. The values of the quadrupole moment associated with the nuclear charge distribution shown above can be inferred from the expression $\int r_n^2\rho_n(3\cos^2\theta_n-1)d\tau_n$ where θ_n is measured from the axis of rotation. In the left-hand figure the charge is concentrated along the angle $\pi/2$ for which the cosine vanishes, and therefore gives a negative value for the integral. In the right-hand figure, the charge is concentrated along the angle 0 (or π) and the expression $3\cos^2\theta_n-1$ is positive, giving a positive value for the integral.

[5] J. A. Stratton, *Electromagnetic theory* (McGraw-Hill, New York, 1941), p. 178ff.

the microwave region. Actually it is found that in many molecules the main absorption line is accompanied by a quadrupole fine structure in which the components are separated by a frequency difference of about 100 megacycles/sec.

In a complete quantum-mechanical treatment of the quadrupole interaction the nuclear and electron charge densities in Eqs. (11) and (12) must be replaced by their appropriate representations in terms of quantum mechanical state functions. When the integrations are then carried out there results the rather complicated expression[6]

$$E_Q=\left(eQ\frac{\partial^2 V}{\partial Z^2}\right)\left[\frac{3K^2}{J(J+1)}-1\right]\times\left[\frac{\frac{3}{4}C(C+1)-J(J+1)I(I+1)}{(2J-1)(2J+3)2I(2I-1)}\right], \quad (19)$$

where I is the nuclear spin quantum number and is equal to the angular momentum of the nucleus in units of $h/2\pi$, F is the sum of the angular momentum of the nucleus I and the angular momentum J due to the rotation of the molecule; that is,

$$F=J+I,\ J+I-1,\ \cdots|J-I|. \quad (20)$$

The quantity C is defined by the relation

$$C\equiv F(F+1)-I(I+1)-J(J+1), \quad (21)$$

and $\partial^2 V/\partial Z^2$ is the second derivative with respect to the coordinate Z along the axis of the molecule of V, the electrostatic potential of all extranuclear charges at the nucleus. The permitted transitions are those for which the total angular momentum remains constant or changes by one unit, that is

$$\Delta F=0,\quad \pm 1. \quad (22)$$

There are two situations in which E_Q can definitely be said to be zero. If the nuclear spin I is equal to 0 or $\frac{1}{2}$ the quadrupole moment vanishes and therefore, from Eq. (19), E_Q is zero. If $J=0$, Eq. (19) is no longer valid. For this case E_Q is also zero.

Before attempting to discuss the interpretation of experimental results in the light of the previous statements, it will be well to examine a brief outline of the experimental techniques involved.

[6] H. B. G. Casimir, *Archives du Musée Teyler* VIII, Sec. III, p. 210ff. See also J. Bardeen and C. H. Townes, *Physical Rev.* **73**, 97 (1948).

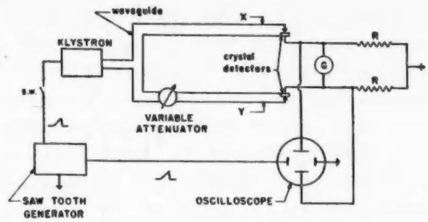

FIG. 5. Schematic diagram of the apparatus used by Good for the detection of microwave absorption lines. Measurements are made by comparing the attenuation in the gas-filled upper arm of the wave guide with that introduced by the variable attenuation in the evacuated lower arm.

If one compares the apparatus of microwave spectroscopy with that of infra-red spectroscopy one finds that the klystron corresponds to the infra-red source, a section of wave guide or a cavity resonator to the collimating mirrors and the crystal detector to the thermocouple.

The klystron differs from the infra-red source in that while the infra-red source supplies continuous radiation whose frequency range is large compared to the absorption band being measured, the klystron radiation has a narrow frequency range comparable to that of the absorption bands in the microwave region. This means that for any extended set of measurements, the frequency of the klystron must be varied. Another difference lies in the fact that while the radiation from an infra-red source is incoherent (i.e., of random phase) that of the klystron is coherent. Therefore, in microwave spectroscopy one is bothered by problems arising from standing waves, or in other words, impedance mismatch in the system.

The klystron tubes used in microwave spectroscopy are generally only slightly larger, or about the same size, as a conventional metal radio tube. Most of them are of the so-called reflex type. This type consists of a single resonant cavity whose frequency is approximately that of the output from the tube. Electrons from the filament are accelerated by a d.c. potential of a few hundred volts and enter the resonant cavity where they are formed into bunches by the radiofrequency field in the cavity. The bunches pass out through the opposite end of the cavity and approach the repeller electrode which is maintained at a negative potential. The electron bunches then reverse their direction and re-enter the cavity where they are decelerated by the

radiofrequency field. While being decelerated, the electrons impart the energy acquired from the d.c. accelerating potential to the radiofrequency field in the cavity. The radiofrequency energy is brought out of the cavity by a coaxial transmission line passing through the base of the tube. Changing the negative potential on the repeller electrode changes the phase of the bunches of electrons which re-enter the cavity and shift the resonant frequency of the tube slightly. The frequency shift is nearly linearly proportional to the repeller voltage, and so by applying a voltage with a sawtooth wave form the frequency may be swept over the range permitted by the design of the tube.

As indicated previously, either wave guides or cavity resonators may serve as absorption cells to contain the gas for producing a microwave spectrum. In either case, the gas pressure must be fairly low in the cell to avoid pressure broadening of the absorption lines. Therefore to obtain a measurable attenuation the wave guide must be fairly long. To avoid excessive attenuation in the metal walls of such a long guide the walls may be made of silver or be silver-plated. Similar considerations are valid for cavity resonators. Since the transition probability per unit time is proportional to the energy density, the cavity must be designed to obtain high field intensities and low losses. Since well designed cavities have relatively narrow bandwidths, they are generally used only for examining the fine structure of lines.

The crystal detectors used are of the silicon type which was developed for radar work during the war. Unlike the thermocouples used in infrared spectroscopy, their output may be fed directly into a.c. amplifiers. The modulation envelope which the crystal detector transmits to its amplifier is determined by the varying voltage applied to the repeller of the klystron. If the absorption cell between the crystal detector and the klystron is empty, and if the system can be adjusted to be free of frequency-sensitive impedance mismatches, the modulation envelope from the detector is proportional to the power emitted by the klystron as its repeller voltage varies over its range. If a saw-tooth voltage is applied to the repeller the frequency of the radiofrequency power absorbed by the crystal varies nearly linearly with time. If a gas is now let into the cell with absorption lines in the frequency range covered by the klystron the output of the crystal will drop at such times as the voltage on the repeller becomes correct for the emission of frequencies corresponding to those of the absorption lines.

While a direct method like that just described has been used, it appears that a comparison method such as that used by Good[7] in the study of the inversion spectrum of ammonia is somewhat more satisfactory. The essential components of Good's system are shown schematically in Fig. 5. The system might be called a "microwave Wheatstone bridge." If unequal amounts of power arrive at the two crystals which terminate branches X and Y of the wave guide, unequal currents will flow through the resistors R and the galvanometer G will show a deflection. The branch X is filled with the absorbing gas and the attenuation in the evacuated branch Y is adjusted by means of the variable attenuator to obtain a zero deflection on the galvanometer at a frequency at which no absorption lines are to be expected. As the klystron's frequency is varied by changing the voltage on the repeller, the bridge will become unbalanced at those frequencies at which the absorption suddenly increases. For continuous and rapid observation the voltage from a sawtooth generator is applied to the repeller and to the horizontal plates of an oscilloscope. The horizontal sweep of the oscilloscope is then proportional to the frequency emitted by the klystron. If the voltage across the galvanometer is applied to the vertical plates of the oscilloscope, the structure of the absorption line being studied is reproduced on the screen of the oscilloscope.

An interesting method has been developed for determining the frequency of satellite absorption lines.[8] A small a.c. voltage whose frequency is of the order of a few megacycles per second is superimposed on the sawtooth voltage applied to the repeller. This causes small "ghosts" or side bands to appear near an absorption line on the oscilloscope trace. Since these "ghosts" are separated from the main line by the frequency of the modulating signal or a multiple thereof, the fre-

[7] W. E. Good, *Physical Rev.* **70**, 213 (1946).
[8] Dr. W. Gordy informs us that this method is due to M. W. P. Strandberg.

quency separation of the satellites can be found by comparing their distance from the main line with that of the ghosts.

We next consider the problem of interpreting microwave spectra. As shown in Table I, a large number of the molecules which have been investigated by microwave methods contain nuclei having quadrupole moments. The resulting spectra are rather complicated and their interpretation is somewhat more difficult than that of atomic hyperfine spectra. On the other hand, the quadrupole interaction enables us to determine the spin and approximate quadrupole moment of the nucleus which produces the splitting.

The general appearance of microwave spectra is indicated by Fig. 6 which shows the energy level diagram and resulting spectrum which arises from the quadrupole splitting of the

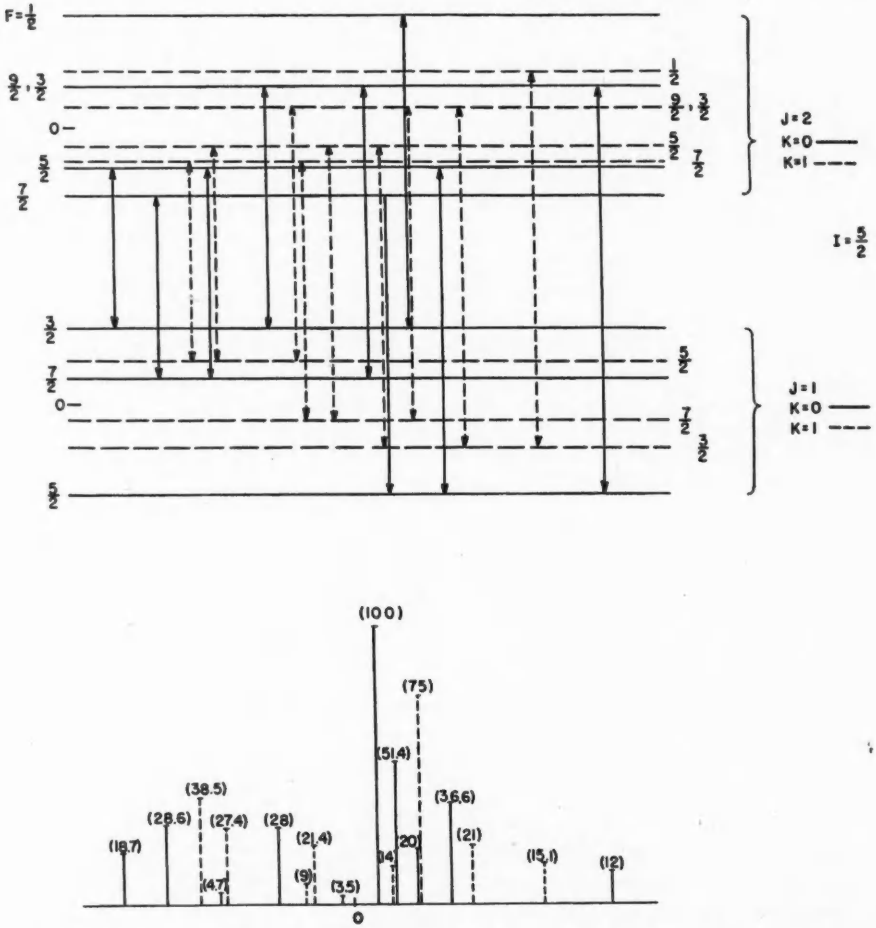

FIG. 6. The CH_3I spectrum for the transition $J=1\rightarrow J=2$. The upper portion of the figure represents the energy levels of $J=1$ and $J=2$ states which are split into many components due to quadrupole interaction. The vertical solid lines are the $K=0$ transitions, and the dashed lines the $K=1$. The double arrowheads indicate transitions which may be accompanied either by absorption or emission. The lower portion of the figure represents the expected absorption spectrum, with the relative intensities indicated by the heights of the vertical lines. The center of this spectrum, indicated by the symbol zero, is the hypothetical frequency at which absorption would take place if there were no quadrupole splitting in either upper or lower level. The upper levels corresponding to $K=2$ have been omitted since the selection rule $\Delta K=0$ does not permit transitions from these levels to lower levels.

TABLE II. Nuclear constants.

Molecule	Nucleus	Spin	Quadrupole coupling mc/sec	Quadrupole moment $\times 10^{24}$ cm^2	Ref.
AsF_3	As^{75}	3/2	−235	0.3	*c*
OCS	S^{32}	0	0	0	*l*
	S^{33}	3/2	−2.85	∼−0.05	*m*
	S^{34}	0(?)	<1	<0.002	*l*
	O^{18}	0(?)	<1	<0.004	*l*
ClCN	Cl^{35}	3/2	−83.2	−0.066	*l*
	Cl^{37}	3/2	−65.7	−0.052	*l*
BrCN	Br^{79}	3/2	686.5	0.28	*l*
	Br^{81}	3/2	573.5	0.23	*l*
ClCN, BrCN	N^{14}	1	−3.63	∼0.02	
ICN	I^{127}	5/2	−2420	−0.75	*l*
BH_3CO	B^{10}	3	+3.3	*∼0.08**	*n*
	B^{10}	3/2	+1.55	*∼0.04**	*n*

* These data supplied by W. Gordy.
a Gordy, Simmons, and Smith, *Physical Rev.* **74**, 243 (1948).
b Smith, Ring, Smith, and Gordy, *Physical Rev.* **74**, 370 (1948).
c Dailey, Rusinow, Shulman, and Townes, *Physical Rev.* **74**, 1245A (1948).
d Dakin, Good, and Coles, *Physical Rev.* **70**, 560 L (1946).
e Hillger, Strandberg, Wentink, and Kyhl, *Physical Rev.* **72**, 157A (1947).
f Townes, Holden, and Merritt, *Physical Rev.* **72**, 513 L (1947).
g Coles, Elyash, and Gorman, *Physical Rev.* **72**, 973 L (1947).
h C. I. Beard and B. P. Dailey, *J. Chem. Physics* **15**, 762 L (1947).
i R. T. Weidner, *Physical Rev.* **72**, 1268 L (1947).
j Townes, Merritt, and Wright, *Physical Rev.* **73**, 1334 (1948).
k Townes, Holden, and Merritt, *Physical Rev.* **72**, 740A (1947).
l Townes, Holden, and Merritt, *Physical Rev.* **74**, 1113 (1948).
m C. H. Townes and S. Geschwind, *Physical Rev.* **74**, 626 L (1948).
n Gordy, Ring, and Burg, *Physical Rev.* **74**, 1191 L (1948).

rotational levels $J=1$ and $J=2$ of the molecule CH_3I.[9] The splitting is due to the quadrupole moment of the iodine nucleus which has spin 5/2. There are eighteen lines altogether, half of which are due to $K=0$ (solid) and the other half due to $K=1$ (dotted). The intensities are indicated by the vertical heights of the spectrum lines. The entire spectrum shown in Fig. 6 is about 1000 Mc/sec wide. Expressed in wave numbers, this width is only 0.03 cm^{-1}, which is of the order of the width of one of the unresolved doublets or triplets in the atomic spectrum of iodine.[10] At present it appears that the only feasible method of identifying spectra as complicated as that previously considered is to prepare a number of theoretically possible spectra and compare them with the observed spectra.[11] Since the quantity $eQ\partial^2 V/\partial Z^2$ in Eq. (19) is unknown, only ratios of differences of frequencies may be compared. As a simple example, consider the three lines of the spectrum of CH_3Cl^{35}. Their frequencies are[12]

$$\nu = 26604.57 \text{ Mc/sec}, \tag{23a}$$

$$\nu_2 = 26589.49 \text{ Mc/sec}, \tag{23b}$$

$$\nu_3 = 26570.77 \text{ Mc/sec}. \tag{23c}$$

From these data one finds

$$\begin{aligned}(\nu_1-\nu_2)/(\nu_2-\nu_3)&=0.806,\\ (\nu_2-\nu_3)/(\nu_1-\nu_3)&=0.554,\\ (\nu_1-\nu_2)/(\nu_1-\nu_3)&=0.446.\end{aligned} \tag{24a, b, c}$$

Now the quadrupole interaction always splits a rotational level into at least three levels. Since only three lines instead of six are observed, one of the levels must be unaffected by the quadrupole interaction. As previously indicated, this situation can arise only if $I=0$ or $\frac{1}{2}$, or if $J=0$. The first possibility can be eliminated by recalling that if $I=\frac{1}{2}$, $Q=0$ and therefore the other level would show no splitting. Thus, one level must correspond to $J=0$. Since this yields the lowest possible rotational energy it must be the lower level. Furthermore, since $J=0$, its projection on the figure axis of the molecule is zero, and therefore $K=0$. Since J may change only by one unit, the next rotational level must be $J=1$. The mass number of Cl^{35} is odd and therefore its spin I must be an odd multiple of one-half. Since $I=\frac{1}{2}$ has already been excluded, the first possibility is $I=\frac{3}{2}$. If this is assumed to be the value of I, Eq. (20) gives the possible values of F for the energy levels arising from the splitting of rotational level $J=1$, *viz.*, 5/2, $\frac{3}{2}$, and $\frac{1}{2}$. Similarly, for the level $J=0$, $F=\frac{3}{2}$. The quantities in the square brackets of Eq. (19) may now be computed. Thus the energies in ergs of the levels given by assuming $I=\frac{3}{2}$ are

$$E_1=E_r(J=1)-eQ\frac{\partial^2 V}{\partial Z^2}[0.25000], \quad F=1/2 \tag{25a}$$

$$E_2=E_r(J=1)-eQ\frac{\partial^2 V}{\partial Z^2}[0.05000], \quad F=5/2 \tag{25b}$$

$$E_3=E_r(J=1)-eQ\frac{\partial^2 V}{\partial Z^2}[0.20000], \quad F=3/2 \tag{25c}$$

[9] Gordy, Simmons, and Smith, *Physical Rev.* **74**, 243 (1948).
[10] S. Tolansky, *Proc. Roy. Soc.* **A149**, 269 (1935).
[11] Bardeen and Townes (see reference 5) have prepared certain tables which greatly facilitate such calculations.

[12] This datum is taken from the paper of reference 9.

for the upper levels, and

$$E_0=E_r(J=0)=0, \quad F=\tfrac{3}{2}$$

for the lower level. By applying the selection rule $\Delta F=0, \pm 1$ and Eq. (1) one finds that permitted frequencies are

$$\nu_1'=(1/h)(E_1-E_0)\times 10^{-6} \text{ Mc/sec}, \quad (26a)$$

$$\nu_2'=(1/h)(E_2-E_0)\times 10^{-6} \text{ Mc/sec}, \quad (26b)$$

$$\nu_3'=(1/h)(E_3-E_0)\times 10^{-6} \text{ Mc/sec}. \quad (26c)$$

The ratios of the differences are then

$$\frac{\nu_1'-\nu_2'}{\nu_2'-\nu_3'}=0.8000, \quad \frac{\nu_2'-\nu_3'}{\nu_1'-\nu_3'}=0.556,$$

$$\frac{\nu_1'-\nu_2'}{\nu_1'-\nu_3'}=0.444. \quad (27a, b, c)$$

These agree sufficiently well with the previous experimental values to be considered as justifying the assumption $I=\frac{3}{2}$. To obtain the quantity $eQ\partial^2 V/\partial Z^2$ one may set the experimental frequency differences equal to the theoretical frequency differences in Mc/sec, as follows:

$$\nu_1-\nu_2=15.08=-(1/h)\left(eQ\frac{\partial^2 V}{\partial Z_2}\right)[0.2000], \quad (28a)$$

$$\nu_2-\nu_3=18.72=-(1/h)\left(eQ\frac{\partial^2 V}{\partial Z^2}\right)[0.2500], \quad (28b)$$

$$\nu_1-\nu_3=33.80=-(1/h)\left(eQ\frac{\partial^2 V}{\partial Z^2}\right)[0.4500]. \quad (28c)$$

The average of the three values obtained from these equations is

$$(1/h)eQ(\partial^2 V/\partial Z^2)\times 10^{-6}=75.13 \text{ Mc/sec}. \quad (29)$$

To obtain the quadrupole moment Q, some method must be found for calculating the quantity $\partial^2 V/\partial Z^2$. Up to the present time, no very exact method has been found, although certain approximate methods have been advanced which probably enable one to estimate $\partial^2 V/\partial Z^2$ sufficiently accurately to obtain Q within an error of 10 percent.[13] Table II lists for several nuclei the quadrupole moments which have been obtained from microwave spectroscopy.

Having obtained $eQ(\partial^2 V/\partial Z^2)$ it is now possible to obtain the rotational energy of the level $J=1$. Inserting Eqs. (25a, b, c) in (26a, b, c) and using the experimental frequencies, one finds three equations for $E_r(J=1)$. One of these is

$$26604.57=(1/h)E_r(J=1) \times 10^{-6}+75.13(0.25000). \quad (30)$$

The average of the three relations gives

$$(1/h)E_r(J=1)=26585.77 \text{ Mc/sec}. \quad (31)$$

The moment of inertia I_B about the axis perpendicular to the axis of the molecule may now be calculated by the use of Eq. (3). One finds $I_B=63.11\times 10^{-40}$ g cm^2. From infra-red rotation vibration spectra it is possible to obtain value of the quantity $(6/I_A)-(7/I_B)$. By combining the knowledge of the value of this quantity with the value of I_B just obtained one deduces I_A, the moment of inertia about the figure axis of the molecule. The general configuration of the molecule is the same as that shown in Fig. 2 with the iodine atom replaced by a chlorine atom. From the measured values of I_B and I_A listed in Table I, it is possible to determine the length of the C−Cl and C−H bonds and the angle between the C−H bonds. Although the spectrum used as an example is a very simple one, the general principles indicated in its interpretation and use may be extended with slight modifications to other microwave spectra.

In the next paper we consider the application of microwave methods to the study of the Stark and Zeeman effects in paramagnetics.

[13] C. H. Townes, *Physical Rev.* **72**, 344 (1947).

Learn the ABC of science before you try to ascend to its summit.—Ivan Petrovich Pavlov, addressing the academic youth of Soviet Russia, 1936.

Note on the Presentation of Maxwell's Equations

JOHN P. VINTI AND D. J. X. MONTGOMERY
Ballistic Research Laboratories, Aberdeen Proving Ground, Maryland

WHEN one considers the modifications in Maxwell's equations for free space that have to be made to take into account the existence of charges, one finds that the relativistic formulation of the equations requires the introduction of a conduction current as soon as the existence of charge is postulated. This well-known requirement is a consequence of the introduction of the charge parameter in the interaction term in the Lagrangian for the system of charges plus field, since the charge-current density term appears as a four-vector. Conservation of charge is a consequence of the forms assumed for the field term and the interaction term in the Lagrangian. Such a powerful and fundamental treatment is, however, ordinarily not available for introductory courses in electrodynamics, and it may be worth while to see on elementary grounds how the presence of a charge term in one of the Maxwell equations modifies another of the equations.

The motive for such a treatment lies in the attempt to unify the concepts of conduction or convection current and displacement current. The usual attempt at this unification follows the historical development: the polarization part of the displacement current is shown to be interpretable as a convection current arising from the motion of real electric dipoles; the vacuum term is then dismissed, sometimes with a reference to Maxwell's original attempt to picture dipoles embedded in the ether. It appears to us that a more suitable method of correlating the concepts is to begin with the displacement current as fundamental, and then proceed to interpret conduction current as a phenomenon of the same nature, magnetically, as displacement current, i.e., that the essential feature of a conduction current, magnetically, is the motion of electric fields surrounding the moving charged particles that constitute the conduction current. While this procedure is not a new idea,[1] we have not seen it carried through in a satisfactory manner. Hence, we shall begin with Maxwell's equations for free space, and from the concept of displacement current *deduce* the modification that becomes necessary (that is, the introduction of conduction current) when there are present charges which are conserved and whose electric fields obey Gauss's law. The current will be considered as a stream of point charges, and the transition to the continuum picture then made by a smoothing process. There is considerable justification for this procedure, since a satisfactory operational definition for the charge-current vector seems to require a point charge formulation.[2]

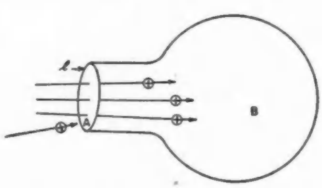

FIG. 1. A set of point charges streams through the loop l which bounds surface A, *into* but *not beyond* the region bounded by B.

Statement of Equations

Let $\mathbf{E}$ and $\mathbf{H}$ denote, respectively, the electric and magnetic field strengths, ρ the charge density, $\mathbf{i}$ the conduction or convection current density, Q the total charge inside a fixed closed surface, I the total conduction current through a closed loop, I_{cl} the total outward conduction current from a closed surface, and c the usual ratio of units. Then, in the Gaussian system, the equations to be considered are:

	Integral Form	*Differential Form*
I. Gauss's law	$\int_{cl} \mathbf{E}\cdot d\mathbf{S} = 4\pi Q$	$\nabla\cdot\mathbf{E} = 4\pi\rho$
II. Ampere's law	$\oint \mathbf{H}\cdot d\mathbf{r} = (4\pi/c)I$	$\nabla\times\mathbf{H} = (4\pi/c)\mathbf{i}$

[1] See, for example, R. A. Houstoun, *A treatise on light* (Longmans Green, London, 1924), pp. 390–392.
[2] B. Podolsky, *Physical Rev.* **72**, 624 (1947).

III. Maxwell's second equation (without charges)

$$\oint \mathbf{H}\cdot d\mathbf{r} = (1/c)\int_{cap} (\partial\mathbf{E}/\partial t)\cdot d\mathbf{S} \qquad \nabla\times\mathbf{H} = (1/c)(\partial\mathbf{E}/\partial t)$$

IV. Conservation of charge

$$I_{cl} + dQ/dt = 0 \qquad \nabla\cdot\mathbf{i} + \partial\rho/\partial t = 0$$

V. Maxwell's second equation (with charges)

$$\oint \mathbf{H}\cdot d\mathbf{r} = (1/c)\int_{cap} (\partial\mathbf{E}/\partial t)\cdot d\mathbf{S} + 4\pi I/c \qquad \nabla\times\mathbf{H} = (1/c)(\partial\mathbf{E}/\partial t) + 4\pi\mathbf{i}/c.$$

In these equations $d\mathbf{r}$ and $d\mathbf{S}$ denote vector elements of arc length and surface area, respectively, $\oint$ denotes a line integral, and the subscripts *cl* and *cap* on an integral sign denote integrals, respectively, over a closed surface or a surface spanning a closed loop. The sign conventions are the usual ones. The above equations are for the vacuum, polarization effects not being germane to the present discussion.

Now it may be shown that each of the following combinations of assumptions: Eq. I or IV, plus Eq. V; Eq. I or IV, plus Eqs. II and III; Eqs. I and IV, plus Eq. II or III implies rigorously, or at least strongly suggests, the remaining members of the set of five equations.[3] In this paper we shall show that the assumption of Eqs. I, III, and IV leads to Eq. V, and thence, trivially, to Eq. II.

Derivation

Figure 1 shows a loop l around which our line integral will be taken, spanned by a surface A of minimum area, and by a much larger surface B, the two together forming a closed surface over which our surface integrals will be taken. A set of point charges in otherwise free space[4] streams through the surface A bounded by l into the region bounded by B, which is taken so large that no charges pass through it during the time under consideration.

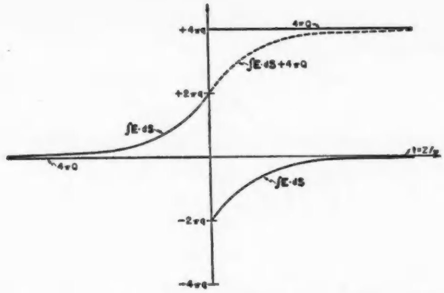

FIG. 2. Electric flux $\int_A \mathbf{E}\cdot d\mathbf{S}$, charge Q (multiplied by 4π), and their sum, through a circle penetrated by point charge q moving with constant velocity v along the Z axis.

Since no charge passes through surface B, Eq. III is applicable; thus

$$\oint_l \mathbf{H}\cdot d\mathbf{r} = (1/c)\int_B (\partial\mathbf{E}/\partial t)\cdot d\mathbf{S} = (1/c)(\partial/\partial t)\int_B \mathbf{E}\cdot d\mathbf{S}, \quad (1)$$

whence

$$\int_0^t dt \oint_l \mathbf{H}\cdot d\mathbf{r} = (1/c)\int_B \mathbf{E}\cdot d\mathbf{S} + \text{const.} \quad (2)$$

Letting *cl* denote the closed surface formed by A and B, we have from Eq. I that

$$\int_B \mathbf{E}\cdot d\mathbf{S} - \int_A \mathbf{E}\cdot d\mathbf{S} = \int_{cl} \mathbf{E}\cdot d\mathbf{S} = 4\pi Q, \quad (3)$$

where Q is the total charge enclosed within *cl*. Then, from Eqs. (2) and (3),

$$\int_0^t dt \oint_l \mathbf{H}\cdot d\mathbf{r} = (1/c)\int_A \mathbf{E}\cdot d\mathbf{S} + 4\pi Q/c + \text{const.} \quad (4)$$

[3] Maxwell's original treatment might be looked upon as assuming Eqs. I, II, and IV, in order to get Eqs. V and III (see, for example, the treatment of M. Mason and W. Weaver in *The electromagnetic field* (University of Chicago Press, 1929), pp. 259–260; or of M. Abraham and R. Becker, *The classical theory of electricity and magnetism* (Blackie, London, 1937), p. 143). In some postulational treatments Eqs. IV and V are assumed, from which Eqs. I, II, and III follow (e.g., J. A. Stratton, *Electromagnetic theory* (McGraw-Hill, New York, 1941), p. 6). Other authors assume Eqs. I and V, from which Eqs. II, III, and IV are derivable (e.g., W. R. Smythe, *Static and dynamic electricity* (McGraw-Hill, New York, 1939), p. 437). R. A. Houstoun, *loc. cit.*, gives the basis for a treatment similar to the present work.

[4] The treatment holds equally well if the moving charges are bound to a material conductor, e.g., if the charges move along a wire penetrating A and terminating in a condenser plate contained within B. The case of an unbroken circuit is handled by considering it as the limit of a circuit with a condenser whose capacitance tends to infinity.

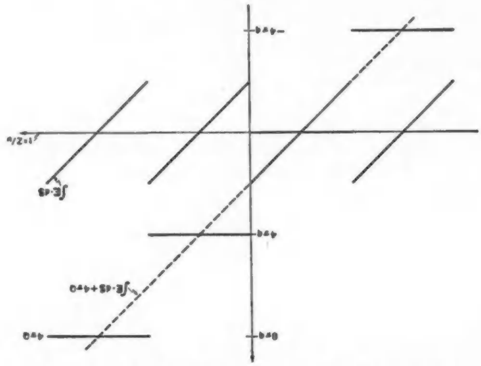

FIG. 3. Electric flux $\int_A \mathbf{E}\cdot d\mathbf{S}$, charge Q (multiplied by 4π), and their sum, through a circle penetrated by a series of equally spaced point charges q moving with constant velocity v along the Z-axis.

At the instant when a point charge q penetrates surface A, the total charge Q within cl increases discontinuously by the amount q, according to Eq. IV. At this instant $\int_A \mathbf{E}\cdot d\mathbf{S}$, the electric flux through A, decreases discontinuously by $4\pi q$, dropping from $\phi'+2\pi q$, just before q crosses A, to $\phi'-2\pi q$ just after q crosses A. Here ϕ' is the flux due to charges not crossing A simultaneously with q. The sum $\int_A \mathbf{E}\cdot d\mathbf{S}+4\pi Q$ is then a continuous[5] function of time, possessing the same properties of continuity and differentiability with respect to time as do the field vectors $\mathbf{E}$ and $\mathbf{H}$.

The considerations of the previous paragraph are exemplified in Fig. 2, which shows a plot of $\int_A \mathbf{E}\cdot d\mathbf{S}$, $4\pi Q$, and their sum, as functions of time t, for the case of a single point-charge q moving along the Z-axis with constant velocity v. At $t=0$, the particle passes through surface A, which is taken as a circle whose center lies on the Z axis, and whose plane is normal to it. The flux $\int_A \mathbf{E}\cdot d\mathbf{S}$ increases with time until $t=0$, when it drops discontinuously from $2\pi q$ to $-2\pi q$, and then increases to zero as the particle recedes from the circle. Meanwhile, $4\pi Q$ has remained zero until $t=0$, when it rises discontinuously to $4\pi q$, and remains at this value for all $t>0$. The two jumps exactly cancel, and the sum is continuous and differentiable. The time derivative of the sum, to which the magnetic field at the periphery of the circle is proportional, is monotonically positive, attaining its maximum value as the particle passes through the plane of the circle. Except at this instant, the magnetic field may be calculated simply from the displacement current, since the conduction current is zero.

Although neither of the variable terms on the right-hand side of Eq. (4) is strictly differentiable, it is possible ordinarily to average over the discontinuities in the usual way to determine derivatives of "smoothed values".[6] Then Eq. (4) becomes

$$\oint_l \mathbf{H}\cdot d\mathbf{r} = \frac{1}{c}\frac{\partial}{\partial t}\int_A \mathbf{E}\cdot d\mathbf{S}+\frac{4\pi}{c}\frac{dQ}{dt}$$

$$=\frac{1}{c}\int_A \frac{\partial \mathbf{E}}{\partial t}\cdot d\mathbf{S}+\frac{4\pi}{c}I. \quad (5)$$

Some insight into the situation described in the preceding paragraph is furnished by examination of the case of a long line of equally spaced particles, each of which carries charge q and moves along the Z-axis with constant velocity v through a circle centered on and normal to the axis. As shown in Fig. 3, each time a particle passes through the circle, the flux through the circle drops discontinuously from $2\pi q$ to $-2\pi q$, rising between successive passages to the full positive value approximately as shown. Meanwhile $4\pi Q$ jumps by $4\pi q$ at each passage. The sum (dashed line) is continuous and differentiable. The time derivative of the sum, to which the magnetic field at the periphery of the circle is proportional, is nearly constant. Except for the slight fluctuations from variations in displacement current due to the assumed granular nature of the conduction current, the magnetic field may be calculated simply by multiplying the (smoothed) value of the conduction current by the proper constant. The displacement current thus disappears from the equation, although in one sense it is just this quantity which gives rise to the magnetic field, the conduction current merely serving to remove the discontinuities.

The deduction of Eq. V from Eqs. I, III, and IV is now complete. The quantity $(1/4\pi)\times\int_A(\partial\mathbf{E}/\partial t)\cdot d\mathbf{S}$ is to be interpreted as the displacement current through the loop l, and I as the conduction current through it. It is important to note that the role of conduction current passing *through one given surface* (A) has been determined with the aid of displacement current *through a different surface* (B). Thus, while we have shown that the magnetic effects of conduction current as well as of displacement current are traceable to the moving electric fields surrounding point charges, we have equally shown that in the macroscopic picture the conduction current must be introduced explicitly when moving charge crosses a surface over which integrals are to be taken.

[5] Obviously we can define the value of the sum at the instant when q penetrates A, as the limit of the sum as q approaches it from either side.

[6] We do not wish to go into this matter in detail because of its length. The important point is that whenever a current can be usefully defined by a smoothing process, Eq. (5) follows from Eq. (4). An indication of the factors involved is suggested by the example following.

On the Planck Radiation Formula

D. PARK AND H. T. EPSTEIN
University of Michigan, Ann Arbor, Michigan

ON at least three occasions the development of quantum theory has been impeded by the occurrence of apparently nonphysical ideas in the structure of the theory. In the beginning it was the quantum of energy, disavowed by Planck until experiments on the photoelectric effect made it clear that the idea has physical content. Then it was the electron with negative energy and positive charge which arose as a by-product of Dirac's theory and which was vaguely identified with the proton until Anderson discovered the positron. The third such phenomenon, an example of which is the subject of this note, is the so-called zero-point fluctuation of the quantum-mechanical fields. The theoretical existence of such an effect is made necessary by the uncertainty relations which the fields must satisfy; in the case of the electromagnetic field, which we shall consider here, a typical one would be[1]

$$\Delta E_x \Delta H_y \geqslant hc/(\delta l)^4$$

which relates ΔE_x, the uncertainty in the value of a component of the electric field measured within a region whose linear dimension is given by δl, with ΔH_y, the corresponding uncertainty in the magnetic field, assuming that the most probable value of **E** and **H** is zero. In this case the entire values of **E** and **H** are those of $\Delta\mathbf{E}$ and $\Delta\mathbf{H}$, and if we assume that ΔH_y is on the average numerically equal to ΔE_x, and drop the subscripts, we get

$$(\Delta E)^2 \geqslant hc/(\delta l)^4.$$

The total energy due to ΔE in the volume $(\delta l)^3$ will be $\Delta W \geqslant hc/4\pi\delta l$. Suppose we now think of the Fourier component of the fluctuations at a particular angular frequency ω. This disturbance may be confined to a region of the order of c/ω in size, so that with $\delta l \sim c/\omega$,

$$\Delta W \geqslant \tfrac{1}{2}\hbar\omega$$

where the factor $\frac{1}{2}$, as derived here, is not to be taken very seriously. This relation states that it is impossible to ascertain the energy of the electromagnetic field at the frequency ν to within the amount ΔW, and that in particular this residual value remains when all external fields, light quanta, and other electromagnetic effects have been removed. A quantum-mechanical calculation shows that the equality sign is correct in the last formula.

Of course this *zero-point energy* has no direct physical consequences. For one thing, when integrated over all frequencies, it is infinite, and would hence produce an infinite gravitational field and other absurdities. Indeed, by a simple change in the quantum-mechanical formula for the field energy,[2] amounting simply to a shift in the zero-level of the energy scale, this constant quantity can be disposed of. However, the possibility remains that the fluctuations of the force vectors **E** and **H** about their average value of zero may not be fictitious, but may give rise to observable physical effects. That this is indeed so is shown by Welton[3] in his recent discussion of the "Lamb shift," the discrepancy found by Lamb and Retherford[4] between the levels of the hydrogen atom as predicted by Dirac's theory and as found by experiment. Of course, this explanation is inherent in the elementary quantum-mechanical treatment of the effect,[5] but it is particularly illuminating to see it calculated semi-classically on the assumption that the fluctuating electric field gives rise to a random vibration of the electron in space. Thus, it seems simplest to consider the zero-point energy as real, though unobservable, since it is merely a constant value added to any observable energy, and to use it as an index of the strength of the fluctuating electric and magnetic fields, which, though not directly observable because they are so small, may yet have directly observable physical consequences.

As a particularly simple consequence of this kind, we consider Planck's formula for the density

[1] W. Heisenberg, *The physical principles of the quantum theory* (Chicago, 1930), p. 50.

[2] W. Heitler, *The quantum theory of radiation* (Oxford University Press, London, 1936 or 1947), p. 60.

[3] T. A. Welton, *Physical Rev.* **74**, 1157 (1948).

[4] W. E. Lamb and R. C. Retherford, *Physical Rev.* **72**. 241 (1947).

[5] H. Bethe, *Physical Rev.* **72**, 339 (1947).

of blackbody radiation, taking as our model the derivation given by Einstein.[6] The difficult point in Einstein's discussion has always been the necessity for assuming the existence of two separate processes by which a system in an excited state can emit a quantum of energy: spontaneous emission, which is what the term implies, and stimulated emission, which is brought about by the presence of radiation at the frequency or frequencies which the system is capable of emitting, and which is thus to be regarded as a sort of resonance effect. Both of these processes occurred in the earliest form of quantum mechanics. Spontaneous emission was treated by the rather heuristic expedient of trying to describe in quantum language something exactly analogous to the classical radiation from an oscillating dipole or multipole, and stimulated emission, also along the lines of the classical model, turned up, for example, in the quantum theory of dispersion. In 1927 Dirac showed that by applying quantum-mechanical principles to the complete system comprising both matter and radiation, one can free oneself entirely from classical analogies and derive all the results from first principles. In this theory, the distinction between spontaneous and stimulated emission does not appear; all radiation, being merely an exchange of energy between parts of a coupled system, is properly regarded as stimulated.

The derivation of Planck's formula for the spectral distribution of radiation in equilibrium with matter can be carried out on the basis of this quantum theory of emission and absorption,[7] but the calculation depends on the details of the quantization of the radiation field, and the simplicity of the result is rather lost sight of.

The situation needs to be examined only at a particular frequency ν. We consider a material system capable of existing in a ground state a and an excited state b having energies ϵ such that $\epsilon_b-\epsilon_a=h\nu$, in equilibrium with radiation of density ρ, and represent ρ as the sum of ρ_0, the energy density due to the zero-point fluctuations, and ρ_1, that due to the real quanta present. We consider all emission as induced and postulate that the components ρ_0 and ρ_1 are equally effective in bringing about transitions from the excited state to the ground state. Thus, if N_b is the population of state b and E is a coefficient of emission independent of ρ and the temperature T, the rate of transitions downward is given by $N_bE(\rho_0+\rho_1)$. On the other hand, transitions upward cannot be caused by ρ_0, since such a process would involve the subtraction of energy from that part of the radiation field which is already in its lowest energy state. Thus, introducing an absorption coefficient A, we can write the condition for radiative equilibrium as

$$N_bE(\rho_0+\rho_1)=N_aA\rho_1,$$

or

$$\rho_1=\frac{\rho_0}{N_aA/N_bE-1}.$$

By Boltzmann's law, applied to the material system, $N_a/N_b=e^{h\nu/kT}$, and further, in order that as T becomes larger the radiation density shall approach infinity, we must take A equal to E, which is nothing but the adoption of Kirchhoff's law. We thus arrive at the simple formula

$$\rho_1=\frac{\rho_0}{e^{h\nu/kT}-1}.$$

The expression for ρ_0 is easily found. The radiation field has two degrees of freedom, and in the lowest state each has an energy of $\frac{1}{2}h\nu$. The statistical weight associated with the frequency ν is $8\pi\nu^2/c^3$, so that we have finally

$$\rho_0=8\pi h\nu^3/c^3,$$

from which follows Planck's law.

[6] A. Rubinowicz, *Handbuch der physik*, XXIV/1, (Julius Springer, Berlin, 1933), p. 55.
[7] W. Heitler, *op. cit.* p. 107.

Human knowledge and human power are coextensive; for ignorance of causes prevents us from producing effects. Nature can be ruled only by being obeyed; for the causes which theory discovers give the rules which practice applies.—FRANCIS BACON.

The Quantum-Mechanical Problem of a Particle in Two Adjacent Potential Minima II. Solution by Perturbation Theory Methods*

D. S. CARTER** AND G. M. VOLKOFF
University of British Columbia, Vancouver, British Columbia

THE problem was stated and an exact solution in implicit form was given in Sec. 1 of Part I of this paper. The implicit results were reduced to explicit form in three special cases in Secs. 2 to 4 of Part I. We now reproduce two of these results by perturbation theory methods, discuss the formal results, and apply them to a model of the ammonia inversion spectrum.

5. λ-Type Perturbation Theory of the Two-Well Problem

In determining the effect which the presence of a shallow potential well has on the levels of a neighboring potential well, we shall make use of the usual Schrödinger perturbation theory discussed in most texts on quantum mechanics. Although most texts do not explicitly consider the case in which the unperturbed problem has a continuous spectrum, their discussions may be easily generalized to include this case.

Let $V_A(x)$ and $V_B(x)\equiv\lambda\bar{V}_B(x)$, respectively, be the potential of the unperturbed problem and the perturbing potential defined by

$$V_A(x)=\begin{cases}-U & \text{for } |x|<a\\ 0 & \text{for } |x|>a,\end{cases} \tag{41a}$$

$$\bar{V}_B(x)=\begin{cases}-U & \text{for } |x-a-2b-c|<c\\ 0 & \text{for } |x-a-2b-c|>c.\end{cases} \tag{41b}$$

Clearly $V_A(x)+V_B(x)\equiv V(x)$ of Eq. (1). Let $H_A(x)$, $H_B(x)$, and $H(x)$ be the Hamiltonian operators of the two unperturbed single-well problems and of the two-well problem, respectively. Then

$$H_A\equiv-(1/\kappa)(d^2/dx^2)+V_A, \tag{42}$$

$$H_B\equiv-(1/\kappa)(d^2/dx^2)+V_B, \tag{43}$$

$$H\equiv-(1/\kappa)(d^2/dx^2)+V_A+V_B\equiv H_A+\lambda\bar{V}_B. \tag{44}$$

Let

$$\phi_\beta\equiv C_A\phi_A\equiv\phi_A(a+1/\beta)^{-\frac{1}{2}} \tag{45}$$

where ϕ_A is defined by Eq. (14a), be the normalized single-well eigenfunction belonging to the unperturbed bound level $E_\beta=E_A<0$. The properly normalized wave functions $\phi_k^\pm$ of the continuum ($E_k>0$) are given by Eqs. (18).

For sufficiently small λ, the eigenvalue E and the eigenfunction ϕ of the two-well problem belonging, respectively, to E_β and ϕ_β are assumed to have the forms

$$\phi\equiv\phi_\beta+\lambda\phi_1+\lambda^2\phi_2+\cdots, \tag{46}$$

$$E\equiv E_\beta+\lambda E_1+\lambda^2E_2+\cdots. \tag{47}$$

The coefficients of these series may be found from standard perturbation theory, expressed in terms of the "matrix elements"

$$\bar{V}_{pq}=\int_{-\infty}^{+\infty}\phi_p\bar{V}_B\phi_q dx=-U\int_{a+2b}^{a+2b+2c}\phi_p\phi_q dx, \tag{48}$$

with

$$p,\,q=\beta,\,\beta',\,k^+,\,k^-,\text{ etc.}$$

Thus

$$E_1=\bar{V}_{\beta\beta}, \tag{49}$$

$$\phi_1=\sum{}'\frac{\bar{V}_{\beta\beta'}\phi_{\beta'}}{E_\beta-E_{\beta'}}+\int_0^\infty\frac{\bar{V}_{\beta k^+}\phi_{k^+}+\bar{V}_{\beta k^-}\phi_{k^-}}{E_\beta-E_k}dk, \tag{50}$$

and

$$E_2=\sum{}'\frac{\bar{V}_{\beta\beta'}{}^2}{E_\beta-E_{\beta'}}+\int_0^\infty\frac{(\bar{V}_{\beta k^+})^2+(\bar{V}_{\beta k^-})^2}{E_\beta-E_k}dk, \tag{51}$$

where the primed summation signs indicate summation over all the bound states of the problem whose eigenvalues $E_{\beta'}$ differ from E_β.

The evaluation of E_1 is a straightforward elementary integration which immediately yields expression (40a). In evaluating E_2, the range of integration of the integral in Eq. (51) is first extended from $-\infty$ to $+\infty$, and then contour integration is used. Investigation of the poles of the integrand (for details see Appendix 4) shows that there are terms from the integral which ex-

* Part I of this paper on the direct solution of the problem appeared in *Am. J. Physics* 17, 187 (1949). The sections, references, equations, figures, and appendices are numbered consecutively throughout both parts.

** Holder of a Studentship from the National Research Council of Canada. Now at Princeton University.

actly cancel out the terms of the summation. The balance of the integral involves only the constants α and β of the level E_β, and is identical with expression (40b). Thus, it is shown that the results of perturbation theory agree exactly, as they should, with the results obtained by direct methods. A discussion of the ranges of validity of the assumed power series expansions (46) and (47) is given later.

6. ε-Type Perturbation Theory of the Two-Well Problem

When the distance $2b$ between the wells is infinite, $V(x)$ reduces to $V_A(x)$ for all finite x, and the two-well problem reduces to the problem of the single well A. If, however, we had initially chosen the origin of the x-coordinate at the center of well B, the two-well problem would have reduced when $b\rightarrow\infty$ to the problem of the single well B. Accordingly we assume that as $b\rightarrow\infty$, every eigenvalue E of the two-well problem approaches a level E_0 of either the single well A or the single well B. Further, if E_0 is a level of one well, say well A, but not of the other, we assume that the normalized eigenfunction ϕ belonging to E has the form

$$\phi(x,b)\equiv\phi_\beta(x)+\psi(x,b), \tag{52}$$

where $\phi_\beta(x)$ is defined by Eq. (45), and $\psi(x,b)$ is a function whose maximum numerical value approaches zero as $b\rightarrow\infty$ (i.e., ψ approaches zero uniformly in x as $b\rightarrow\infty$). On the other hand, if E_0 is a level of both single wells, we assume that

$$\phi(x,b)\equiv A\phi_A(x)+B\phi_B(x,b)+\psi(x,b), \tag{53}$$

where $A\phi_A+B\phi_B$ is a linear combination of the eigenfunctions of wells A and B belonging to E_0, and ψ approaches zero uniformly in x as $b\rightarrow\infty$.

We may now use perturbation theory to solve the two-well problem approximately if we assume that for every single-well eigenvalue $E_0=E_\beta$ (say of well A), there is an eigenvalue E of the two-well problem which is expressible as a power series in the parameter ϵ (defined by Eq. (20) above) of the form

$$E=E_\beta+\epsilon E_1+\epsilon^2E_2+\cdots. \tag{54}$$

If E_β is not a level of both single wells, we may substitute Eqs. (52) and (54) into the two-well Schrödinger equation to obtain

$$(H_A+V_B)(\phi_\beta+\psi)\equiv(E_\beta+\epsilon E_1+\epsilon^2E_2+\cdots)(\phi_\beta+\psi). \tag{55}$$

Here we regard V_B as the perturbing operator, ψ as the perturbation of the wave function, and $\epsilon E_1+\epsilon^2E_2+\cdots$ as the perturbation of the energy level.

In this problem, however, we cannot employ standard perturbation theory since the perturbing potential well $V_B(x,\epsilon)$, which moves off to infinity when the expansion parameter $\epsilon\rightarrow0$, is not expressible as a power series in ϵ. Moreover, the function ψ need not be expressible as a power series in ϵ. We need only assume that the function

$$\bar{\psi}=\psi/\epsilon \tag{56}$$

is bounded as $\epsilon\rightarrow0$.

Rewriting the identity (55) with the aid of $(H_A-E_A)\phi_A\equiv0$, we find that

$$(\epsilon E_1+\epsilon^2E_2+\cdots)(\phi_\beta+\psi)\equiv(H_A-E_\beta)\psi+V_B(\phi_\beta+\psi). \tag{57}$$

Multiplying both sides of this identity by ϕ_β, integrating over all x, and dividing by ϵ, we obtain an identity in ϵ:

$$(E_1+\epsilon E_2+\cdots)\int_{-\infty}^{+\infty}\phi_\beta(\phi_\beta+\psi)dx\equiv\epsilon^{-1}\int_{-\infty}^{+\infty}\phi_\beta V_B(\phi_\beta+\psi)dx. \tag{58}$$

Since ϕ_β and ψ are of order ϵ (or less) near well B (see Eqs. (14) and (56)), and E_1 is independent of ϵ, we find on taking limits as $\epsilon\rightarrow0$ that the first-order correction vanishes. That is,

$$E_1=0. \tag{59}$$

In order to determine E_2, we set $E_1=0$ in Eq. (58), divide again by ϵ, and let $\epsilon\rightarrow0$. We obtain

$$E_2=\lim_{\epsilon\rightarrow0}\left\{\epsilon^{-2}\int_{-\infty}^{+\infty}\phi_\beta V_B(\phi_\beta+\psi)dx\right\}. \tag{60}$$

Before we can evaluate expression (60) we must find an explicit limiting approximation to $\phi=\phi_\beta+\psi$ which is correct to terms of order ϵ within well B, where V_B differs from zero. Equations (44) and (59) show that as $\epsilon\rightarrow0$, Eq. (55) reduces to

$$H_A\phi=[E_\beta+0(\epsilon^2)]\phi \tag{61}$$

in the regions to the left of well B, and to

$$H_B\phi=[E_\beta+0(\epsilon^2)]\phi \tag{62}$$

in the regions to the right of well A. Equation (61) shows that in the neighborhood of well A, ϕ may, in fact, be closely approximated by the normalized single-well eigenfunction ϕ_β in agreement with the assumption concerning ψ made following Eq. (52). Further, in accordance with that assumption, and with the fact that ϕ_β is itself of order ϵ inside well B, we see that ϕ must also be of order ϵ inside well B. The precise form which ϕ takes inside well B is determined by Eq. (62). However, we shall show that to terms of order ϵ, ϕ may be replaced in regions to the right of well A by the improper single-well wave function χ_B of well B, belonging to the energy value E_β, and of suitable amplitude B. The wave function χ_B is defined as that solution of

$$H_B\chi_B=E_\beta\chi_B \tag{62a}$$

which approaches zero as $x\rightarrow+\infty$. Since E_β is an eigenvalue of well A, and by hypothesis is therefore *not* a simultaneous eigenvalue of well B, χ_B necessarily diverges as $x\rightarrow-\infty$, and is therefore an improper wave function of well B; it is explicitly given by Eqs. (4d) to (4f) and the analogs of Eqs. (6). Since ϕ must be of order ϵ inside well B, the amplitude B in Eqs. (4d) to (4f) must be of order ϵ. This fact enables us to replace the solution ϕ of Eq. (62) by the solution χ_B of Eq. (62a) to terms of order ϵ. To determine the value of B explicitly to terms of order ϵ, we join the diverging term of χ_B smoothly to ϕ_β at any point between the two wells. The expression for ϕ_β for $x>a$ is found from Eqs. (14), (15), (16), and (45) to be

$$\phi_\beta=\pm(\alpha/\kappa U)(a+1/\beta)^{-\frac{1}{2}}\exp-\beta(x-a). \tag{63}$$

Accordingly, the value of B obtained by identifying expression (63) with that exponential term of expression (4d), which diverges as $x\rightarrow-\infty$, is given by

$$B=\frac{1+\gamma_B}{\gamma_B}\cdot\frac{e^{-2\beta b}}{\cos\alpha'(c+\delta')}\cdot\frac{\pm\alpha}{(\kappa U(a+1/\beta))^{\frac{1}{2}}}, \tag{64}$$

where γ_B is evaluated at E_β, and the sign is chosen in agreement with that in expression (63).

Equation (60) now gives for E_2,

$$E_2=\lim_{\epsilon\rightarrow0}\left\{-\epsilon^{-2}\lambda U\int_{a+2b}^{a+2b+2c}\phi_\beta\chi_B dx\right\}. \tag{65}$$

Substituting ϕ_β from Eq. (63), χ_B from Eqs. (4e) and (64), integrating, and reducing the result with the aid of Eq. (15) and the analogs of Eqs. (6) and (7), we obtain

$$E_2=-\frac{2\lambda\alpha^2\beta^2}{\kappa(1+\beta a)(\beta^2-\alpha'^2+2\alpha'\beta\cot2\alpha'c)}=\frac{1}{\gamma_A'\gamma_B}, \tag{66}$$

in complete agreement with the coefficient of ϵ^2 in expressions (23) and (29b).

In the special, but physically important case of exact degeneracy, when E_β is a level of both single wells, we find a first-order splitting in agreement with Eq. (29a). Substituting Eqs. (53) and (54) into the Schrödinger equation for the two-well problem, we find

$$H(A\phi_A+B\phi_B+\psi)\equiv(E_\beta+\epsilon E_1+\epsilon^2E_2+\cdots)(A\phi_A+B\phi_B+\psi). \tag{67}$$

Substituting $H\equiv H_A+V_B$, multiplying by ϕ_A, integrating over all x, dividing by ϵ, and letting $\epsilon\rightarrow0$, we also find

$$B\epsilon^{-1}\int_{-\infty}^{+\infty}\phi_AV_B\phi_Bdx-AE_1\int_{-\infty}^{+\infty}\phi_A{}^2dx=0. \tag{68a}$$

Similarly, using $H\equiv H_B+V_A$, and multiplying by ϕ_B, we obtain

$$A\epsilon^{-1}\int_{-\infty}^{+\infty}\phi_BV_A\phi_Adx-BE_1\int_{-\infty}^{+\infty}\phi_B{}^2dx=0. \tag{68b}$$

The condition for the existence of simultaneous solutions for A and B from Eqs. (68a) and (68b) is

$$\begin{vmatrix} -E_1\int_{-\infty}^{+\infty}\phi_A{}^2dx, & \epsilon^{-1}\int_{-\infty}^{+\infty}\phi_AV_B\phi_Bdx \\ \epsilon^{-1}\int_{-\infty}^{+\infty}\phi_BV_A\phi_Adx, & -E_1\int_{-\infty}^{+\infty}\phi_B{}^2dx \end{vmatrix}=0. \tag{69}$$

From Eqs. (14) to (16) and their analogs for well B we find by elementary integration that

$$\begin{aligned}\epsilon^{-1}&\int_{-\infty}^{+\infty}\phi_AV_B\phi_Bdx\\ &=\epsilon^{-1}\int_{-\infty}^{+\infty}\phi_BV_A\phi_Adx=\pm2\alpha\alpha'/\kappa^2U\lambda^{\frac{1}{2}}\\ &=\pm C_A{}^{-1}C_B{}^{-1}(\gamma_A'\gamma_B')^{-\frac{1}{2}},\end{aligned} \tag{70}$$

where γ_A' and γ_B' are evaluated at E_β. In accordance with Eqs. (70) and Eq. (15) and its analog, Eq. (69) reduces to

$$E_1 = \pm(\gamma_A'\gamma_B')^{-\frac{1}{2}} \tag{71}$$

in complete agreement with the coefficient of ϵ in Eqs. (24) and (29a). Finally, we obtain from Eqs. (68a), (15), (70), and (24)

$$\left|\frac{B}{A}\right| = \frac{|E_1|}{C_A{}^2} C_A C_B(\gamma_A'\gamma_B')^{\frac{1}{2}} = \frac{C_B}{C_A}, \tag{72}$$

which agrees with Eq. (36).

We conclude our perturbation theory discussion by using our results for the case of exact degeneracy to obtain results for the case of approximate degeneracy. If $E_\beta = E_A$ lies very close to a level E_B of well B, it is clear that we may bring one of these levels into coincidence with the other by making a certain small change of order $E_A - E_B$ in the depth of either well, and thereby obtain an exactly degenerate problem whose levels differ from those of the original problem by terms of order $E_A - E_B$. Hence, for values of b such that the level shifts of the new problem are much greater in numerical value than $|E_A - E_B|$, these shifts will be good approximations to those of the original problem. But our perturbation theory results tell us that for large values of b, the level shifts for the *new* problem are approximated by quantities of the form of expression (24). Moreover, it is clear that these quantities differ by terms of order $E_A - E_B$ from

$$\Delta = \pm\epsilon[\gamma_A'(E_A)\gamma_B'(E_B)]^{-\frac{1}{2}}, \tag{73}$$

where ϵ belongs either to E_A or to E_B, and γ_A' and γ_B' belong to the *original* problem. If, then, there are values of $\epsilon \ll 1$ for which

$$|\Delta| \gg |E_A - E_B|, \tag{74}$$

we expect that the quantities Δ of expression (73) are good approximations to the required level shifts $E - E_A \approx E - E_B$ of the original problem.

7. Discussion

To illustrate the range of validity of the λ-type perturbation theory we consider a two-well problem in which the wells are a constant distance apart, well A is of depth U and of such width as to have three bound levels E_{A0}, E_{A1}, and E_{A2}; and well B is of slightly narrower width than well A, and of variable depth λU. The positions of the levels of both single-well problems and of the combined two-well problem are sketched in Fig. 3 as functions of λ. The horizontal dashed lines represent the levels of well A, and the horizontal dot-dashed lines represent the infinite discontinuities of $\gamma_A(E)$, which are all independent of λ. The sloping dot-dashed straight lines represent the infinite discontinuities of γ_B, and the sloping dashed curves, approaching the straight lines as asymptotes, represent the bound levels of well B. The heavy solid curves represent the two-well levels. From Eq. (8) it is clear that for finite separation of the wells ($\bar{\epsilon}=0$), γ_A is zero if, and only if, γ_B is infinite, and vice versa. Accordingly, the heavy curves must cross the discontinuous curves at the encircled points, which mark the intersections of the single-box levels with the infinite discontinuites $E_{\infty,n}$, and can cross the discontinuous curves only at these points.

The approximate results for the case of large separation of the wells (small ϵ) obtained both by the direct method of Sec. 3 and by the ϵ-type perturbation theory method of Sec. 6 are illustrated in Fig. 3 by the fact that the heavy curves closely follow the dashed curves. In accordance with Eqs. (23) and (24), which have also been reproduced by perturbation theory methods, the heavy curves lie farther (at distances of order ϵ) from the nearest dashed curves near the points of intersection of two dashed curves (where "degenerate" single-well levels occur), than at other points (where the distances from the dashed curves are of order ϵ^2).

The approximate results for small λ, obtained by both the direct method of Sec. 4, and by the λ-type perturbation theory of Sec. 5, are illustrated by the behavior of the three heavy solid curves which closely follow E_{A0}, E_{A1}, and E_{A2} before their first turning points. It is clear that when b is large, the curves turn so sharply that the series (39) and (47) cannot be expected to be valid beyond the first turning point of each curve, and certainly the approximations given by the first two terms of the series will not be valid beyond these points. Further, since the coefficients of the series depend upon b, and the curves must pass through the encircled points regardless of the value of b, it is clear that these approxima-

tions, and probably the series themselves, will never be valid up to the first encircled point of each curve. Thus the λ-type perturbation theory applied to the various levels of well A will give reliable results only as long as λ is such that all levels of well B lie above the particular well A level under consideration.

To illustrate the range of validity of the ϵ-type perturbation theory applied to the physically important completely degenerate case of two identical wells, we consider an example in which the wells are of such width that $a(\kappa U)^{\frac{1}{2}}=3.5$, and plot in Fig. 4 the dimensionless quantity $x=\alpha a=a(\kappa E+\kappa U)^{\frac{1}{2}}$, which determines the levels E of the two-well problem, as a many-valued function of ϵ. The continuous curves represent the exact values of x. In accordance with Eqs. (7), (8), and (21) each continuous curve has an equation of the form

$$\left|\frac{y^2-x^2+2xy\cot 2x}{3.5^2}\right|^{y/y_0}=\epsilon, \qquad (75)$$

where

$$y=\beta a=(3.5^2-x^2)^{\frac{1}{2}}, \qquad (76)$$

and y_0 is the value of y at the appropriate single-well level (where $\epsilon=0$).

The dashed curves represent the first-order approximations to the exact curves in accordance with Eq. (29a) and the approximation

$$\Delta x=a\Delta\alpha\approx a\kappa\Delta E/2\alpha \qquad (77)$$

obtained from Eq. (5a).

The range of applicability of the approximation for the lower levels is surprisingly large. For the lowest level, for instance, the approximation holds very well from $\epsilon=0$ to $\epsilon\approx 0.4$. The value of y_0 for this curve is $y_0=0.313$; and hence, we find from Eq. (21) that the value of b/a corresponding to $\epsilon=0.4$ is

$$b/a=-(\log\epsilon)/2y_0\approx 0.14. \qquad (78)$$

That is, the approximation is good for values of b between infinity and $0.14a$.

8. Application of the Model to the Ammonia Inversion Spectrum

We conclude by using the simple model of two neighboring identical rectangular wells to illustrate the origin of the inversion spectrum of ammonia, and the method of obtaining some constants of the ammonia molecule from this spectrum.

Many authors[4] have pointed out that the motion of the NH_3 molecule which contributes to the inversion spectrum is that in which the nitrogen atom moves back and forth through the triangle formed by the three hydrogen atoms. There is an equilibrium position for the nitrogen atom on either side of the triangle, and a potential barrier with a maximum in the plane of the triangle which the nitrogen atom must traverse.

It has been shown[6] that the method of normal coordinates may be used, and hence, although the molecule is three-dimensional, the levels of the inversion spectrum closely approximate the levels of a one-dimensional, two-minimum problem. Many authors have used this fact to estimate some constants of the ammonia molecule. Manning,[8] for instance, assumed a potential function similar to that shown on the right of Fig. 5; and by assuming a reduced mass $\mu=4.6\times 10^{-24}$ g, and fitting the three separations of the lowest four levels of his model to those found from the ammonia spectrum, he determined the "equilibrium height of the NH_3 pyramid" (half of the separation of the minima of the potential function), the height of the "potential hump" between the minima, and the asymptotic value of the potential function at large distances from the minima. He then calculated some of the higher levels, and found them to be in good agreement with those of ammonia.

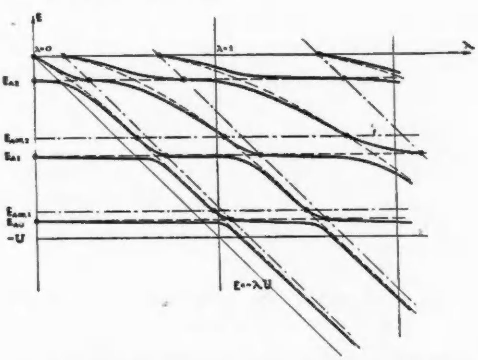

FIG. 3. The dependence of the levels of a particular two-well problem on the depth of one of the wells. Solid curves represent the levels of the two-well problem, dashed lines represent the unperturbed single-well levels, and dot-dashed curves represent the "infinite-well" levels.

TABLE I. Levels of NH_3 inversion spectrum (cm^{-1}).

Designation	Experimental	Manning	Square wells with infinite sides
0^+	0	0	0
0^-	0.66	0.83	0.83
1^+	932.4	935	935
1^-	968.1	961	961
2^+	1597.5	1610	1640
2^-	1910	1870	2170
3^+	2380	2360	2650
3^-	2861	2840	3290

In our calculations we first assume a two-well potential function with two identical square wells and three adjustable parameters a, b, and U, which we determine by using the same reduced mass and making the same fit as Manning. Our numerical method is first to use the equations

$$y^2-x^2+2xy\cot 2x=0 \quad \text{and} \quad y^2+x^2=M^2 \qquad (79)$$

to determine by graphical means the values of $x=\alpha a$ and $y=\beta a$ for the lowest two levels of the single-well problem as a function of $M=a(\kappa U)^{\frac{1}{2}}$. Then assuming the approximation (29a) to be valid, we notice that if ΔE is the splitting of one of these levels when the boxes are at a distance $2b$

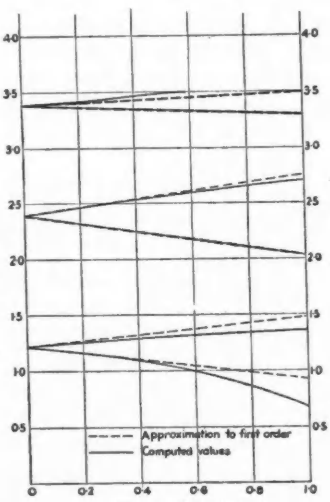

FIG. 4. The dependence of $x=a[\kappa(E+U)]^{\frac{1}{2}}$ on $\epsilon=\exp[-2b(-\kappa E_0)^{\frac{1}{2}}]$ for two identical wells with $a(\kappa U)^{\frac{1}{2}}=3.5$.

apart, then

$$\epsilon=\Delta E\kappa^2 U(1+\beta a)/4\alpha^2\beta^2$$
$$=\kappa a^2\Delta E M^2(1+y)/4x^2y^2. \qquad (80)$$

But if x_0 and x_1 are the values of x for the first two single-well levels, we have:

$$x_1^2-x_0^2=a^2\kappa(E_1-E_0) \qquad (81)$$

and hence,

$$\epsilon=\frac{\Delta E}{E_1-E_0}\,\frac{M^2(1+y)(x_1^2-x_0^2)}{4x^2y^2}. \qquad (82)$$

Thus, by using the observed values of $\Delta E/(E_1-E_0)$ for the first and second pair of ammonia levels, we calculate one value of ϵ for each of the split levels, and then find the two corresponding values of

$$b/a=-(\log\epsilon)/2y. \qquad (83)$$

By trying different M's and interpolating, we find one for which the two ratios are equal. Finally, using this M, and the assumed μ, we calculate a, b, and U.

We find that our value of the "equilibrium height of the NH_3 pyramid" $(a+b)$ agrees very well with Manning's, and that our height of the "potential hump" (U) agrees fairly well. However, with this model of the potential function we find no higher bound levels. Therefore, we next assume a potential function as shown on the left of Fig. 5. After finding the necessary equations for this model, and making calculations similar to those outlined above, we obtain the results shown in Fig. 5, and listed in Tables I and II.

In order to compare our model with Manning's we used the same numerical values for the four lowest levels that he did, even though the later more accurate experimental values[14] given in the second column of Table I are slightly different.

In Table II the height of the potential hump and the asymptotic height, $V(\infty)$, of the potential function are given with respect to the minimum value of $V(x)$, and not with respect to the lowest level.

We wish to thank Professor E. Hill of the University of Minnesota for valuable discussion. One of us (D.S.C.) wishes to thank the National Research Council of Canada for the award of a

[14] Taken from G. Herzberg, *loc. cit.*, Table 74, p. 297.

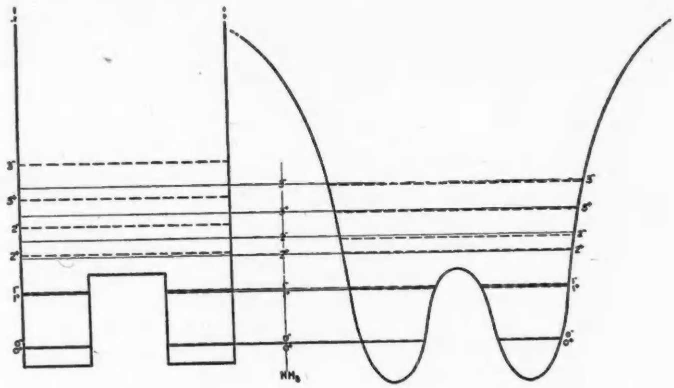

FIG. 5. Comparison with the experimental values of the levels of the ammonia inversion spectrum calculated on the square-well and Manning's models.

Studentship during tenure of which this work was done.

Appendix 4. Evaluation of E_1 and E_2 in the Perturbation Theory Expressions (49) and (51)

We find E_1 from Eqs. (14), (15), (16), and (48)

$$E_1=\bar{V}_{\beta\beta}=-\frac{\alpha^2}{\kappa(a+1/\beta)}\int_{a+2b}^{a+2b+2c}e^{-2\beta(x-a)}dx, \quad \text{(A25)}$$

which on elementary integration directly yields Eq. (40a).

To find E_2 we first use Eqs. (14), (15), and (48) to evaluate by elementary integration

$$\bar{V}_{\beta\beta'}{}^2=(\epsilon\epsilon'\alpha\alpha')^2\beta\beta'[1-e^{-2(\beta+\beta')c}]^2/\kappa^2(\beta+\beta')^2(1+\beta a)(1+\beta' a), \quad \text{(A26)}$$

where α', β', ϵ' are the values of α, β, ϵ corresponding to the bound state with $E=E_{\beta'}$. Similarly, from Eqs. (14), (15), (18), and (48) we find the matrix elements

$$|\bar{V}_{\beta k\pm}|=\frac{U\alpha}{(\pi\kappa U(a+1/\beta))^{\frac{1}{2}}}\times\left|\int_{a+2b}^{a+2b+2c}e^{-\beta(x-a)}\cos k(x+\delta^{\pm})dx\right|. \quad \text{(A27)}$$

Writing the cosine function in exponential form, integrating, and using the relation

$$E_\beta-E_k=-(\beta^2+k^2)/\kappa=-(k+i\beta)(k-i\beta)/\kappa, \quad \text{(A28)}$$

we find

$$(\bar{V}_{\beta k\pm})^2/(E_\beta-E_k)=N(T_1+T_2+T_3+T_4{}^{\pm}+T_5{}^{\pm}), \quad \text{(A29)}$$

where

$$N=\epsilon^2\alpha^2\beta U/4\pi(1+\beta a), \quad \text{(A30)}$$

$$T_1(k)=2e^{2(ik-\beta)c}/(k+i\beta)^2(k-i\beta)^2, \quad \text{(A31)}$$

$$T_2(k)=T_1(-k), \quad \text{(A32)}$$

$$T_3(k)=-2(1+e^{-4\beta c})/(k+i\beta)^2(k-i\beta)^2, \quad \text{(A33)}$$

$$T_4{}^{\pm}(k)=e^{2ik(a+2b+\delta^{\pm})}\times[e^{2c(ik-\beta)}-1]^2/(k+i\beta)^3(k-i\beta), \quad \text{(A34)}$$

$$T_5{}^{\pm}(k)=T_4{}^{\pm}(-k). \quad \text{(A35)}$$

With the aid of Eqs. (A32) and (A35) and the fact that $T_3(k)=T_3(-k)$, we find that the integral in Eq. (51) takes the form

$$I=\int_0^\infty\frac{(\bar{V}_{\beta k+})^2+(\bar{V}_{\beta k-})^2}{E_\beta-E_k}dk$$
$$=N\int_{-\infty}^{+\infty}(2T_1+T_3+T_4{}^{+}+T_4{}^{-})dk. \quad \text{(A36)}$$

We evaluate this last integral by means of integration in the complex plane around the contour consisting of the real axis from $-R$ to $+R$ and the semicircle of radius R in the upper half of the k plane. Since the integral along the semicircle vanishes as $R\to\infty$, we obtain

$I=2\pi iN\times$(the sum of the residues of the integrand at its poles in the upper half plane). (A37)

Expressions T_1 and T_3 have poles of the second order at $k=i\beta$ only. The residue of $2T_1$ is

$$R_1=4\left[\frac{d}{dk}\cdot\frac{e^{2(ik-\beta)c}}{(k+i\beta)^2}\right]_{k=i\beta}=\frac{2\beta c+1}{i\beta^3}e^{-4\beta c}, \quad \text{(A38)}$$

while that of T_3 is

$$R_3=-2(1+e^{-4\beta c})\left[\frac{d}{dk}(k+i\beta)^{-2}\right]_{k=i\beta}$$
$$=-\frac{1}{2i\beta^3}(1+e^{-4\beta c}). \quad \text{(A39)}$$

Adding R_1 and R_3, and multiplying by $2\pi iN$, we find that the contribution of T_1 and T_3 to E_2 is exactly equal to the first term of Eq. (40b).

In order to evaluate the residues of $T_4{}^{+}$ and $T_4{}^{-}$, we find from Eqs. (19) that

$$e^{2ik(a+\delta^{\pm})}=\frac{(k+\alpha)e^{2i\alpha a}\pm(k-\alpha)}{(k-\alpha)e^{2i\alpha a}\pm(k+\alpha)}, \quad \text{(A40)}$$

and hence from Eqs. (A40) and (7) that

$$e^{2ik(a+\delta^{+})}+e^{2ik(a+\delta^{-})}=\frac{-2\kappa U}{k^2+\alpha^2+2ik\alpha\cot 2\alpha a}=\frac{2}{\gamma_A(E_k)}. \quad \text{(A41)}$$

This equation tells us that $T_4{}^{+}+T_4{}^{-}$ has simple poles at each of the points $k=i\beta'$, where β' is the value of $\beta=(-\kappa E)^{\frac{1}{2}}$

TABLE II. Shapes of potential functions.

	Manning	Square wells	Square wells with infinite sides
Width of boxes	—	0.28A	0.36A
Separation of boxes	—	0.44A	0.41A
Equilibrium height of pyramid	0.37A	0.36A	0.38A
Height of potential hump	2071 cm^{-1}	1640 cm^{-1}	1650 cm^{-1}
$V(\infty)$	45,100 cm^{-1}	1640 cm^{-1}	∞

for any bound level other than the one whose shift is being investigated; and a pole of order two at the point $k=i\beta$ corresponding to the level under consideration. The residue at each $k=i\beta'$ is given by

$$R_4(i\beta')=\left\{2e^{4ik\beta}[e^{(2ik-\beta)c}-1]^2\Big/ (k+i\beta)^3(k-i\beta)\gamma_A'(E_k)\frac{dE_k}{dk}\right\}_{k=i\beta'}. \quad \text{(A42)}$$

Evaluating this residue with the aid of Eqs. (5) and (12), applying Eq. (A30), and comparing with Eq. (A26), we find that

$$2\pi iNR_4(i\beta')=-\bar{V}_{\beta\beta'}{}^2/(E_\beta-E_{\beta'}). \quad \text{(A43)}$$

Thus, every term in the primed summation of Eq. (51) which arises from a discrete level $E_{\beta'}$, is canceled by an equal and opposite term which comes from the integral due to the levels of the continuum.

Finally, we evaluate the residue of $T_4^{+}+T_4^{-}$ at its second-order pole $k=i\beta$

$$R_4(i\beta)=-\tfrac{1}{2}\left[\frac{d^2f}{dk^2}\Big/\left(\frac{df}{dk}\right)^2\right]_{k=i\beta} \quad \text{(A44)}$$

where

$$f(k)=\tfrac{1}{2}(k+i\beta)^3\gamma_A(E_k)e^{-4ikb}/[e^{2(ik-\beta)c}-1]^2. \quad \text{(A45)}$$

Using the equation

$$d^2\gamma_A/dk^2=\gamma_A''(dE_k/dk)^2+\gamma_A'd^2E_k/dk^2, \quad \text{(A46)}$$

where primes denote differentiations with respect to E, we obtain with the aid of Eqs. (5)

$$R_4(i\beta)=\frac{i\epsilon^2}{4\beta^3\gamma_A'}[e^{-4\beta c}-1]^2 \times\left\{\frac{2\kappa b}{\beta}-\frac{\gamma_A''}{2\gamma_A'}+\frac{\kappa}{\beta^2}\left[1+\frac{2\beta c}{1-e^{4\beta c}}\right]\right\}. \quad \text{(A47)}$$

With the help of Eqs. (A47), (40a), (12), (9), (A30), and the analog of Eq. (7), we find that $2\pi iNR_4(i\beta)$ is exactly the second term on the right-hand side of Eq. (40b).

We note that for large separation of the wells (small ϵ), the matrix elements $\bar{V}_{\beta\beta'}$ between discrete states are of order $\epsilon\epsilon'$, while the matrix elements $\bar{V}_{\beta k\pm}$ between discrete and continuous states are of order ϵ. Therefore, the summation terms in Eq. (51) are of order $(\epsilon\epsilon')^2$, while the integral is of order ϵ^2. The above evaluation of the contour integral showed that the terms T_1 and T_3 in the integrand of Eq. (A36) gave rise to the first term of Eq. (40b) which is indeed of order ϵ^2, while the terms T_4^{+} and T_4^{-} gave rise to a contribution of order $(\epsilon\epsilon')^2$, part of which canceled the summation terms, and part of which was equal to the second term of Eq. (40b). Thus for small ϵ, the states of the continuum contribute more to E_2 through the integral in Eq. (51) than do the discrete levels through the summation, even though the energy denominators are smaller in the summation than in the integral.

Reproductions of Prints, Drawings and Paintings of Interest in the History of Physics

43. *Vanity Fair* Caricature of William Robert Grove

E. C. WATSON
California Institute of Technology, Pasadena 4, California

ANOTHER of the physicists whose caricature appeared early in the remarkable series published in *Vanity Fair* during the last half of the nineteenth century was SIR WILLIAM ROBERT GROVE (1811–1896), the inventor of the voltaic cell that bears his name and the author of *The Correlation of Physical Forces*, a work that helped greatly in establishing the principle of the conservation of energy. This caricature, together with the following account was published on October 8, 1887:

The Hon. Sir William Robert Grove

"Seventy-six years ago there was born to a worthy Justice of the Peace at Swansea an embryo Judge of the High Court, whose name they called WILLIAM ROBERT. As to the details of his early years the oldest history is silent, but we know that he took an Oxford degree some twenty years later, and that he was called to the Bar in 1835. From that time Mr. Grove's career was rendered brilliant, first by galvanic electricity,

and later by scientific exposition of the law on behalf of commercial clients. Soon after he entered upon his profession his health took an indifferent turn, by reason of which he was led away for a time from the strait paths of law to the more fascinating studies of electricity and chemistry, with the happy result that he gave to the world the evil-smelling battery which bears his name. This was a great step towards scientific fame, and Mr. Grove now became a Professor, a Fellow of the Royal Society, a President of the British Association, and an authority on the decomposition of water, the continuity of natural phenomena, the correlation of physical forces, and many other high-sounding luxuries of the scientific world. It was now time for Mr. Grove to return to his first love; which he did, very shrewdly making of his scientific fame a very excellent stepping-stone to Knighthood and the Bench. In those days science was an unknown quantity in the forum, for none of its frequenters had any; but Mr. Grove changed all that, and soon proved to the satisfaction of the litigating public that no patent or other case which might involve any scientific or chemical process was complete without him, a fact which the Government realised in the course of years, and so placed him upon the Bench in 1871—some fifteen years later than he should have been placed there.

"Mr. Justice Grove thenceforth proceeded to dispense a mixture of equal parts of science and law to an ordinary public, with considerable discrimination and some success, and his dispensation was continued until last month, when he retired from public life full of honours and of years. He might have been a better Judge had he been made one earlier in life, but it is no fault of his that this was not the case. He has always been noted for his industry, and for an amount of imperturbable good humour which has made him a general favourite with the Bar, and kept him so, even when his faculties had become slow and his science old-fashioned.

"Galvanic Electricity." [From *Vanity Fair*, October 8, 1887.]

"Sir William is a very nice, agreeable old gentleman of the olden school, who has outlived most of his contemporaries, and everyone now wishes him to enjoy his well-earned repose. With one possible exception, he has never been known to make an enemy, but he has plenty of friends, and deserves them all. It is doubtful whether his name will live longest in law or in science; but it is a fact that all his science never enabled him to master the intricacies of the Judicature Acts. By his retirement he has shown himself to be possessed of much sound sense."

Boner

"*The Moment of Inertia is the instant when a body ceases to start.*"—contributed by ALAN T. WATERMAN.

Recipient of the 1948 Oersted Medal for Notable Contributions to the Teaching of Physics

The American Association of Physics Teachers has made to Professor Arnold Sommerfeld, long time Professor of Physics at the University of Munich, the thirteenth of its annual awards for notable contributions to the teaching of physics. The addresses of recommendation and presentation were made by Professor Paul Kirkpatrick, Chairman of the Committee on Awards, and Professor J. W. Buchta, President of the Association, in a ceremony held in McMillan Hall, Columbia University, on January 28, 1949, during the eighteenth annual meeting.

Arnold Sommerfeld

ADDRESS OF RECOMMENDATION BY PROFESSOR PAUL KIRKPATRICK, CHAIRMAN OF THE COMMITTEE ON AWARDS

EACH year since 1936 the American Association of Physics Teachers, acting through its established committee apparatus, has chosen a physicist deemed to have contributed notably to the teaching of physics, and has sought to do him honor by a public award. This custom has been made possible by the generosity of two of the Association's members. We have come now to the pleasant annual ceremonial through which this award, the Oersted medal and certificate, are bestowed.

The recipient of the current, and thirteenth annual Oersted Award is ARNOLD SOMMERFELD, long-time Professor of Physics at the University of Munich. Arnold Sommerfeld's great career as a teacher of physics began over fifty years ago at the Berg-academy in Klausthal. A subsequent appointment at Aachen was followed in 1906 by a call to the University of Munich, from which base Sommerfeld's world-wide influence has radiated for more than forty years.

The notable contributions of Arnold Sommerfeld to the teaching of physics are to be found in his writings and in the instruction which he imparted to his own students. His great book *Atomic Structure and Spectral Lines* appeared at a critical and dramatic time. All middle-aged physicists must remember its impact. It will be remembered that the papers of Bohr had drawn attention to the important problems of external atomic structure and to the promising spectroscopic method of attacking them. Physics teachers recognized that great revelations were imminent but often found that the Bohr model, like other epoch-making, ground-breaking conceptions, had a way of answering one question and then raising two or more new ones. Into this ferment of physical thought *Atombau und Spektrallinien* brought not the answer to every question but a tremendously valuable consolidation of the gains which had been hurriedly achieved, and a guidebook to the unfamiliar paths of further advance.

Through this book more than any other, the quantum theory was made known to the world. In its several translations it set the tone and direction of advanced instruction in many lands.

In a later period discovery and instruction came again into a time of frustration. Some of the trusted paths of advance threatened to terminate in dead ends. There was an hour of perplexity, of retracing old steps, and of questioning old presumptions. Then came the illumination of wave mechanics. But such illumination is only for the comfort of the elite unless it be distributed by a competent, conscientious, and intelligible messenger. Sommerfeld was such a messenger. His famous *Ergänzungsband*, the second volume of *Atomic Structure and Spectral Lines*, was perhaps the most influential of the early scholarly interpretations of wave mechanics. In many American universities teachers were soon mediating it to their classes far ahead of the appearance of an English translation.

These books have gone through many translations and editions. Probably there is no physicist here who has not been influenced by them. Sommerfeld's textbooks on general theoretical physics are less well known in this country, but they have contributed notably to the teaching of physics abroad, and even now their author is revising them for American publication. So much for the books.

The effectiveness of the teacher is reflected in the achievement of the student. This is not to say that great teaching is the sole condition needed for the production of noteworthy men of science, but when for decades an unbroken series of physicists of the highest caliber issues from the tutorship of one man, it is necessary to conclude either that great training has been going on or that young men of exceptional promise, in large numbers and in many lands, believe that such is the case. From among the scores of physicists who have been trained in whole or in part under Sommerfeld it is easy to list many for whom the surname is quite sufficient identification. Thus Wentzel, Heitler, Houston, Stueckelberg, Ewald, Morse, Pauling, Laporte, Herzfeld, Teller, Brillouin, Eckart, Pauli, Condon, Landé, London, Bethe, Debye, Heisenberg, Laue, and Bragg, each spent one, two, or several years at Sommerfeld's famous institute. No doubt this list could be greatly extended by the inclusion of other familiar names; names of students whose experience at Munich does not happen to be known to me. I certainly have not presumed to draw up here an exclusive list of Sommerfeld's best men.

Several of Sommerfeld's students have kindly written to me of the characteristics of their teacher. These witnesses are all physicists of high renown and I shall quote without identifying them individually as I speak. First, they are agreed in their general estimates of Sommerfeld's quality as a teacher of physics. Quotations follow:

"It seems to me he has been one of the most influential physics teachers of the period between the two world wars." (W. V. Houston)

"There cannot be any doubt about the fact that Sommerfeld is one of the greatest teachers of theoretical physics living." (K. Bechert)

"His ability to attract and encourage young talents, his clear presentation of the significant ideas of physics, and his success in letting the students take part in his own thinking make me consider him one of the greatest living teachers of physics." (Otto Scherzer)

"In bestowing the Oersted medal upon Arnold Sommerfeld the American Association of Physics Teachers is honoring the physicist who, more than any other one man, has influenced the present generation of physicists both abroad and in this country." (Otto Laporte)

". . . from personal experience I have learned that Professor Sommerfeld is one of the greatest teachers of theoretical physics in the world." (L. Pauling)

All teachers of physics will surely be interested in the methods used by one who has achieved such distinguished success, and in the general nature of his genius. Concerning Sommerfeld one of his students comments as follows:

"Characteristic of Sommerfeld's style is the wide use of mathematical methods and philosophical aspects without being hampered by exaggerated mathematical scrutiny or by too vague philosophical speculations. It is characteristic of his way of teaching that many of the problems he discussed in his lectures for advanced students, and in his seminar, were those which he was just going to solve himself. When unexpectedly faced with a serious difficulty he was not afraid to think aloud, thus inadvertently demon-

strating to his students how even a mind like his had to make several futile attempts before finding a suitable way, and how even for him scientific thinking was not only a matter of congenial intuition, but also hard work." (Scherzer)

"His method is not easy to describe: He likes to give explanations by geometrical means, by practical applications of the theorem given. . . . He never showed the attitude of authority in scientific discussions; he liked to discuss the most advanced problems with intelligent students, thus leading them immediately to the very front of his science." (Bechert)

An appreciative student speaks of ". . . the patience with which he prepared the ground of his lectures, making it possible for everyone, even those quite unfamiliar with the subject, to follow him to the frontier of knowledge . . . the boldness with which he used every available mathematical tool once he had led us to the frontier." And this quotation continues: "I have never heard him be sarcastic: the use of this device of the poor teacher, by which the student's ignorance is made a foil for the 'teacher's' supposed erudition, is so foreign to Sommerfeld's nature that he would probably be surprised to have its absence commented upon." (Eckart)

". . . he used to emphasize the problems and difficulties rather than explain them away. He made you feel that science is something alive and that even as a beginner you can be a useful member of this organism." (Wentzel)

"He evaluates carefully the needs and abilities of his audience, and adapts the presentation of his subject accordingly, constantly trying to forget his personal likes and dislikes, his attitude (in discussions with students) giving the impression not of a professor talking with a student but of two equals discussing a most vital and interesting problem." (Debye)

"He had an unusual ability to see through a mass of confusing detail to the main points of a problem, and he conveyed to his associates a feeling for simplicity and elegance in theoretical physics." (Houston)

"In his basic graduate courses he presented the subject with classic clarity." (Laporte)

"Sommerfeld's lectures are masterpieces of clear exposition. He made no concession to expediency in the presentation of a difficult subject, but always tackled it with vigor, presenting a clear, honest discussion that the able students gathered around him could understand. There were no gaps left in his reasoning to puzzle the student. Sommerfeld would point out the places where the theory was still uncertain, in order that the student would know that his failure to understand was due to deficiency in the state of the science and not in his own reasoning ability." (Pauling)

"One characteristic of Professor Sommerfeld which always struck me with admiration is his willingness to display his ignorance. I often remember him saying to someone (myself at times!) 'I'm sorry, can you explain this more fully? I don't grasp these ideas quickly!' It often turned out that what he called his 'lack of understanding' concerned more fundamental aspects which we had not even realized needed investigation, but his open-minded and modest approach was often the most salutary and inspiring part of the episode. I am glad to join in greeting a great teacher, who not only was a lucid transmitter of knowledge but a personal inspiration as well." (Morse)

Is there much more than this that may be desired of a teacher? Mr. President (addressing Professor Buchta), I have the pleasure and the honor to present the name of Arnold Sommerfeld, world-renowned teacher of physics, as the recipient of the thirteenth Oersted Award of the American Association of Physics Teachers.

The object of the book remains the same as before, namely, to give a comprehensive account, not however too difficult, which will allow the nonacademic reader to enter into the new physics of the atom. . . . An attempt has been made to render the account throughout as vivid as possible.—*Atomic Structure and Spectral Lines* (Preface to third German edition).

Some Reminiscences of My Teaching Career

A. Sommerfeld
Dunantstrasse 6, Munich 23, Germany

LIVING as I am in the evening of my life, I take pleasure in looking back over my long teaching career in theoretical physics, which is closely connected with the tremendous rise of physics in this century. During the years 1908 to 1910 I particularly enjoyed giving special lectures on the theory of relativity, especially in its four-dimensional form, as developed by Minkowski. From 1912 on, it was Bohr's theory that I tried to make clear to my students as well as to myself; after 1926 it was wave mechanics. My first lectures on this theory were heard by Linus Pauling, who learned as much from them as I did myself. In 1927, in my special lectures, I also treated the theory of electrons in metals for the first time, and published them soon afterwards, together with C. Eckart and W. V. Houston. In connection with this, N. H. Frank of Massachusetts Institute of Technology and I were able to report on the complicated thermoelectric and thermomagnetic effects in one of the first numbers of the *Reviews of Modern Physics*. During a brief summer semester I. I. Rabi and E. U. Condon were among my students.

However, I should like to speak here only of those students who wrote their theses under me and are at present in the United States. In this connection should be mentioned first of all P. Debye from Maastrich, who was my assistant in Aachen. When I received a telegram from Röntgen concerning my new appointment, I said: "Debye, we have a call to Munich." He really did not hesitate for one moment to accompany me to Munich, where he began his march of triumph in the fields of physics and chemistry. P. S. Epstein, now in Pasadena, was able during the first World War to enter and leave my Institute at will, even though he was a native of Russia. At that time he wrote the wonderful treatise on the Stark effect. A. Landé originated his famous g-formula, to be sure, without my help, in that he generalized the special cases treated by me; however, he too received his doctor's degree under me. Last but not least, I mention Hans Bethe. That my opinion of him was correct may be seen from the fact that I suggested for his doctor's thesis the observations of Davisson and Germer, whose theory at that time was still in quite an unsatisfactory state.

Personal instruction in the highest sense of the word is best based on intimate personal acquaintanceship. Ski trips with my students offered the best opportunity for that. Munich is situated so close to the mountains that one can reach the ideal ski terrain of the "Sudelfeld" by rail in two hours. There I had a ski hut together with my mechanic Selmayr, the builder of ingenious models of crystal structures, which are also known in the United States. The neighboring hut belonged to my colleague from the Technische Hochschule, J. Zenneck. He and I spent many a weekend there with our candidates for the doctorate. Selmayr always praised especially the good humor of the American students and their willingness to get water, split wood, and wash and dry dishes. I remember well the first bold attempts at skiing of W. P. Allis of Massachusetts Institute of Technology. In the evening when we were gathered around the stove, it was inevitable that our conversation should turn from snow and weather to the subject of mathematical physics.

The years 1920 to 1922 represent an especially remarkable period, for at that time two freshmen came to my Institute: Wolfgang Pauli from

Fig. 1. Arnold Sommerfeld, his wife, and one of his two sons. The background was the new wing of the University of Munich, of which a few fragments are still standing. (Photograph by E. H. Kraus.)

Vienna and Werner Heisenberg from Munich. Pauli, the son of a well known professor of medical chemistry at the University of Vienna, had just finished the Gymnasium at Vienna, but had already secretly, "below the desk," studied Einstein's treatises. Heisenberg's father, professor of Byzantine language and literature at our university, had told me that his son was especially interested in mathematics and physics. At the latter's first conference with me, he told me that he had read Hermann Weyl's book, *Raum, Zeit, Materie*, and that he believed he understood it. I could not help but say to him: "It is a lucky chance that I am going to give a course in elementary mechanics this semester. Just do the exercises diligently; then you will find out what you have understood and what you have not." But during his second semester, when I gave a course in hydrodynamics, I agreed to his publishing a note on vortices in the *Physikalische Zeitschrift*. I said to my colleague Heisenberg: "You belong to an irreproachable family of philologists, you, yourself, being a great expert on the late Greek period, your father-in-law a famous expert on Homer, and now you have the misfortune of seeing the sudden appearance of a mathematical-physical genius in your family." Soon afterwards I published together with the younger Heisenberg a treatment of the intensity of multiplets using the correspondence principle. Something similar occurred in the case of Pauli. I had undertaken the editing of the volume on physics of the *Mathematischen Encyclopädie*. The article on the theory of relativity was still lacking. Since Einstein did not want to write it, I suggested to Pauli that we do it together. But when he showed me the first draft of his essay, it proved to be so masterly that I renounced all collaboration. The work of the 22-year-old is unsurpassed to date. I naturally urged both Pauli and Heisenberg to take part in laboratory work as well. They worked together with their friend Otto Laporte in joint experiments under my colleague W. Wien; Pauli contributed more advice than work in order to avoid a "Pauli effect." For Laporte I brought along from Pasadena reports of the Zeeman effects on the sun by H. D. Babcock, on the basis of which Laporte succeeded in clearing up the iron spectrum.

But in addition to my special students, I also had to attend to the rest of our students. I did that in a 6-semester course, beginning with mechanics and ending with the partial differential equations of physics. In this connection I assumed that the students had already got over the mathematical children's diseases (differential and integral calculus, analytical geometry, and theory of simple functions). I asked some instructors to prepare, among other things, an introduction to vector analysis, which is usually somewhat abbreviated or presented in a form unsuitable for use in physics in mathematical instruction in Germany. I used to organize my own lectures in such a way that they were too easy for advanced students and too difficult for beginners. The lectures were confined essentially to classical physics, which, as a basis for all modern developments, must never be curtailed. In the last few years I have been busy preparing them for publication. To date, they have been, or are in the process of being, printed as far as Vol. V (thermodynamics and statistics). I am glad that the Academic Press Inc., N. Y. is doing an English translation and that they were able to turn over the translation of Vol. VI to Dr. Strauss. Another circumstance that has given me great pleasure is the recognition which my teaching activity has received, as indicated by the presentation to me by my American friends of the Oersted Medal.

After the discovery of spectral-analysis no one trained in physics could doubt that the problem of the atom would be solved when physicists had learned to understand the language of the spectra. So manifold was the enormous amount of material that had been accumulated in sixty years of spectroscopic research that it seemed at first beyond the possibility of disentanglement.—*Atomic Structure and Spectral Lines* (Preface to first German edition).

NOTES AND DISCUSSION

A New Apparatus for Demonstrating an Induced Electromotive Force

HERMANN HAEMMERLE
St. Anton am Arlberg (Tirol), Austria

IT is customary to use the ballistic galvanometer for demonstrating the electromotive force produced by the movement of a coil of wire in a magnetic field. This approach in elementary teaching to the concept of the creation of an electromotive force is by no means direct. It requires previous understanding of the functioning of the ballistic galvanometer, which the beginning student seldom has. Therefore, it seems more advantageous to use a simple electrostatic voltmeter, electrometer, or even an electroscope, which indicates directly the magnitude and the sign of the voltage induced in a moving coil of wire.

I have constructed an apparatus which seems to combine all necessary features for a simple demonstration of induced voltage. The unit consists of three parts, the main features of which may be listed:

1. A powerful, permanent double-magnet M_1M_2 held in soft-iron holders and placed on adjustable supports as shown in Fig. 1. Suitable magnets are available as war-surplus. These are magnetron magnets, weighing 15 lb and giving 5000 gauss flux density in the air-gap. The field produced by these magnets could be varied by installing a U-shaped magnetic shunt. A minimum flux density of approximately 1000 gauss is necessary for satisfactory operation.
2. A flat coil of fine copper wire having 500 or more turns and diameter of 10 cm. This coil shown as C in Fig. 1, is tapped at $\frac{1}{3}$ of its length T.
3. A flat, steel spring B fastened at its upper end by a firm clamp and suspended from a frame so that it can swing freely between the poles of the magnet, with a period approximately $\frac{1}{3}$ sec. The coil is fastened to the lower end of the flat spring.

The apparatus constitutes a small model of the electric generator with its principal parts—magnetic field, armature and driving mechanism.

The apparatus functions as follows: The flat spring is bent sidewise and released. As the coil swings through the magnetic field it makes momentary contact in the median position with a tip-over switch Ct connected to the electrometer or electroscope E. A range of approximately 100 volts is satisfactory for the indicating instrument. The contact is broken immediately after the coil moves beyond the median position, with the result that the electrometer indicates the voltage induced at the instant the coil makes the contact.

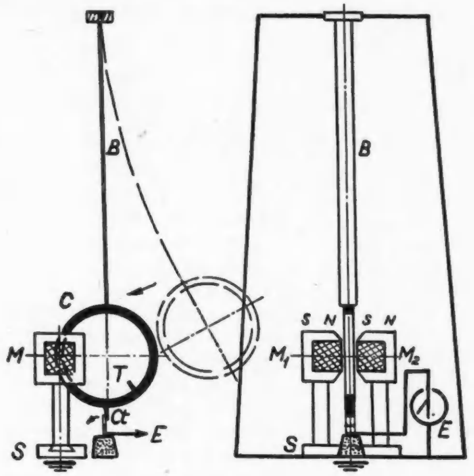

FIG. 1. Apparatus for demonstrating induced electromotive force.

To remove any doubt as to whether the deflection of the electrometer is due to an electrostatic or frictional effect, the experiment may be repeated with the magnets removed. For this condition, of course, the electrometer will show no deflection. The magnitude of the voltage induced by the magnetic field may be determined by calibrating the electrometer with a dry battery. By changing the magnetic field (moving the magnets apart), by varying the amplitude of oscillation, or by using tap T with the smaller number of turns, it is possible to show how the induced voltage varies with magnetic field intensity, speed of coil, and number of turns in the coil. The speed of the coil is approximately proportional to the initial displacement of the spring, and in any case, may be computed from the amplitude and frequency of the oscillation. It may also be determined approximately by mounting the coil on a phonograph disk rotating with a known speed in the magnetic field. The magnetic field intensity may be determined by Cotton's method of weighing the coil when it carries a current in the magnetic field.

The apparatus may also be used for measuring instantaneous velocities: for instance, those of falling bodies. If the coil be allowed to fall between two vertical wires and to make contact with the tip-over switch in the magnetic field, it can be used to determine the speed as a function of height of fall. Similarly, motion on an inclined plane and Newton's Second Law of Motion can be demonstrated by making the coil move along either an inclined or a horizontal track. By fastening the coil to bodies of various shapes one can even study the influence of shape upon the speed attained.

LETTERS TO THE EDITOR

Meson Mass and Range of Nuclear Forces

YUKAWA[1] first pointed out that the existence of particles a few hundred times heavier than the electron ("mesons") is a consequence of the experimental evidence on the finite range of nuclear forces. His argument was formulated in mathematical terms. Later on, Wick[2] gave a physical argument based on the application of the uncertainty principle to the exchange of mesons between nucleons. I wish to present an argument which is complementary to that of Wick and leads directly to the basic theoretical estimate of the meson mass μ through the equation $\mu \sim \hbar/cr_0$ where r_0 is the range of the nuclear forces.

The existence of a radiation associated with any field of force is a general consequence of the relativistic principle that the velocity of propagation of all physical actions is finite and $\leqslant c$. Thus, for example, if an electric charge oscillates with a frequency $\nu=\omega/2\pi$ the electromagnetic field at distances $\ll c/\omega$ from the charge has practically the "static" value corresponding to the position and velocity of the charge at each particular instant. The field at distances $\gg c/\omega$ cannot "keep in step" with the variations of position and velocity of the charge because the effect of these variations propagates only with a velocity c. The signals which tend to adjust the field in *all* the surrounding space to the ever changing position of the charge and travel outwards at distances $\gg c/\omega$ with a velocity c constitute the electromagnetic radiation of frequency ν.

Suppose now (and this is fairly close to truth) that the field of nuclear forces surrounds each nucleon only up to a distance r_0. More generally it is sufficient to assume that the field strength, as a function of the distance, experiences a particularly sharp drop in the proximity of a critical distance r_0. If a nucleon oscillates with a frequency $\nu \ll c/2\pi r_0$ the *whole* nuclear field can keep in step with the variations of position and velocity of the nucleon. There is then no question of a radiation which may travel away from the oscillating nucleon as the electromagnetic radiation travels away from an oscillating charge. This situation is reversed, however, for high frequency oscillations, with $\nu \gg c/2\pi r_0$; and radiation should then be emitted. According to the principles of quantum physics this radiation must then consist of quanta of energy $h\nu \gtrsim hc/2\pi r_0 = \hbar c/r_0$. But the statement that the energy of a kind of energy quanta has a lower limit $\hbar c/r_0$ means essentially that this limit is the minimal or "rest" energy of the quanta; that is, that the quanta have a "mass" $\mu \sim (\hbar c/r_0)/c^2 = \hbar/cr_0$.

Yukawa's treatment showed that in the case of the field of force due to a potential $V(r)=\exp(-r/r_0)/r$ the associated radiation consists of quanta with a uniform rest mass just equal to $\hbar/cr_0$.

U. FANO
National Bureau of Standards
Washington, D. C.

[1] *Proc. Phys. Math. Soc. Japan* **17**, 48 (1935).
[2] *Nature* **142**, 993 (1938), also presented by L. Rosenfeld, *Nuclear Forces* (Interscience, New York, 1948), Vol. I, p. 13.

One Concept of Pressure

IN the article "Pressure Never Has Direction" Summers[1] expressed one concept of the term pressure. Physicists are agreed that pressure is calculated by the equation $p=f/a$, and that pressure is expressed in such units as g/cm². But all physicists apparently do not agree as to the physical understanding of the term pressure. Of course, a law or fact of nature is not involved. The point of discussion is what physicists understand by the term pressure. It is a matter of definition; but it is important that physicists agree on the meaning of the term, and then use it in that sense.

One concept of pressure, which is at variance with the concept presented by Summers, is that a pressure is a force, and the value of the pressure is the value of the force that is exerted on unit area.

When one quantity is divided by another quantity, the concept of the quotient is sometimes, but not always, totally different from either of the original quantities. Newton's second law states $a=f/m$ in the usual notation. The acceleration of the body is a very different concept from the force that acts on the body having an inertia m. In fact, this equation states a law of nature, that is, a relationship between the cause of the motion, the kind of motion and a property of the body being moved.

But an equation does not always express the relationship between different concepts. The equation $p=f/a$ is a definition of pressure, stating that pressure is the force on unit area and therefore is a vector in the same direction as the force. Take the case of a liquid at rest, the bottom of the container being 4 cm² in a horizontal position. There is a force down on each of these 4 cm² of the container and that force is, by definition, the pressure on the bottom. It is expressed in units, such as g/cm² which by definition means grams of force on 1 cm². If the liquid did not exert a force

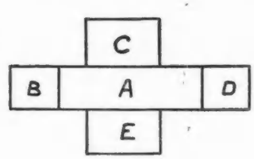

FIG. 1. Four blocks on the same horizontal plane. A vise pushes blocks B and D against block A; another vise pushes C and E against A.

down on each square centimeter, it would not exert a force down on all four square centimeters. The bottom of the container however need not be as large as one square centimeter. For example, it may be ½ cm², and yet there will be a force on the bottom due to the liquid, and the pressure on the bottom is also spoken of. In this case, what is meant by pressure is the force that would be exerted on a square centimeter, if the force on each half of this square centimeter were the same as the force on the bottom of the container, whose area is ½ cm².

It would perhaps be a more logical treatment in so far as units are concerned, if pressure on a surface were defined as the force on the surface divided by a number giving the ratio between the number of unit areas to the unit area; for example, when the horizontal bottom of the container has an area of 4 cm², the pressure,

$$p=\frac{F}{4\ \text{cm}^2/1\ \text{cm}^2}=\frac{F}{4}.$$

In this treatment pressure and force have the same units, and one would have to look at the context to know on what area the force F is being exerted, and also what the size of the unit area is on which the force p is being exerted. This treatment gives exactly the same physical concept of pressure as is described in the preceding paragraphs. This method of defining pressure, however, is not being advocated, as it seems unnecessarily cumbersome, though it does have some points of clarity in its favor.

The fact that a small volume of liquid exerts a force in all directions at the same time is in accordance with our common experience when dealing with larger objects. Consider a block A in Fig. 1. A vise pushes B and D against A, and another vise pushes C and E against A. Thus at the same time, A pushes to the right and to the left, as well as forward and backward. If A were spherical, and if at each point on its surface an outside body exerted a force on A, then A would exert a force in all directions at the same time. This force may be expressed in terms of pressure. If A is infinitesimally small in a liquid at rest, A exerts the same pressure in all directions.

Hence one concept of pressure is that it is the force which is exerted on unit area. Using this concept, it is correct to refer to downward pressure, horizontal pressure, etc., as well as to refer to the pressure which the liquid exerts on the bottom of the tank. Used in this sense, pressure is a vector quantity.

VOLA P. BARTON

Goucher College
Baltimore, Maryland

[1] R. D. Summers, *Am. J. Physics* **14**, 311–313 (1946).

Comments upon "One Concept of Pressure"

THE usual criterion for general acceptance of a definition in physics is that the quantity so defined be useful, both in the immediate solution of practical problems, and as a foundation for further consistent development of other concepts. In the following cases, Barton's definition of pressure leads us into difficulties:

1. The pressure tensor reduces to a scalar quantity in simple cases, never to a vector quantity. It would certainly be undesirable to have fundamentally different concepts of pressure in elementary and in advanced work.

2. If p represents the pressure, and v the volume of a substance, $\int pdv$ equals, in the case of a reversible process, the external work of expansion. Volume and work are scalar quantities. If pressure is a vector quantity, we have thus a scalar quantity equal to the product of a vector quantity and a scalar quantity, an impossible situation. We might reconstruct the formulas of thermodynamics to conform with Barton's concept of pressure, but that concept is not what is now called pressure in $\int pdv$.

3. To explain her concept of a multidirectional pressure at a point within a fluid, Barton pictures a block which is exerting forces of elastic reaction in four directions simultaneously. In the writer's opinion, when the block becomes a sphere, the four forces become not one force, but an infinite number of infinitesimal forces in different directions, outward from the elements of area of the sphere. These separate forces cannot be considered as a single force which acts in all directions, because force is a vector quantity and can have only one direction at a given place and instant. For the same reason, we cannot define pressure as a vector quantity. There is no such thing as a vector quantity which, at a given point, has all directions at once.

4. For "a more logical treatment in so far as units are concerned," Barton eliminates the units of area from pressure, so that "pressure and force have the same units." This procedure is not consistent with the conventional treatment of units, in that it yields a quantity, pressure, whose units do not reveal what units of area were used, although the magnitude of the pressure depends on the units of area.

R. D. SUMMERS

Western Maryland College
Westminster, Maryland

A Problem about Moving Charges

THE article by Han Kong-Chi under the above title,[1] in which he discusses the forces between two electrons moving abreast on parallel courses, interested me as I have made use of this problem in a rather different way.

A consideration of the forces involved, assuming instantaneous fields, shows that the apparent discrepancy is most pronounced at speeds comparable to that of electromagnetic waves, so that it seems natural to introduce the idea of fields propagating at this speed. This leads to the equations for the forces, given as Eqs. (3) and (3′) in Han Kong-Chi's paper.

Students who can follow the argument so far will already be familiar with the Lorentz transformations as deduced, for example, to explain the results of the Michelson-Morley experiment; and they should experience no difficulty in obtaining the relations connecting the accelerations (Eq. (5) in reference 1) directly from these transformations.

Division of the force equations, as obtained from the electrical problem, by the acceleration equations then produces the result

$$m=m_0(1-\beta^2)^{-\frac{1}{2}}, \tag{1}$$

where m is the mass of a particle whose rest mass is m_0, traveling with a velocity β times the velocity of light.

This is certainly the simplest method of which I am aware of deriving this important equation, and I have found that students accept it readily. The method does not appear

artificial since they know that the majority of cases where Newtonian mechanics are insufficient involve atomic or subatomic particles, such as are dealt with in this problem. It should be added that the method is not original, as Jeans[2] has used a very similar method to obtain this equation.

There is one point, however, on which this analysis needs further explanation. The forces and accelerations involved are transverse to the velocity, and in consequence the mass m of Eq. (1) is called the "transverse mass." By essentially similar arguments, Jeans[2] deduces a different relation for the "longitudinal mass," which is involved when the force and acceleration are in the direction of the velocity. This introduction of two different masses for one particle arises from the carry-over into relativity of the equation $F=ma$. If, instead of this, Newton's second law is written

$$F=d(Mv)/dt, \tag{2}$$

then it is found that the distinction between longitudinal and transverse mass is no longer necessary. Since a transverse acceleration does not initially alter the speed, it does not alter the mass, so that for transverse acceleration it is legitimate to bring M outside the differentiation, and its identity with the transverse mass can thus be established. By this argument the extension of Eq. (1) to apply to the mass M in Eq. (2) can be justified.

The argument is not rigorous, as it does not verify that Eq. (2) is applicable to longitudinal accelerations. It is possible to do this by modifying the statement of the original problem, and the solution, but the gain in rigor is heavily outweighed by the added complexity, whereas the chief merit of the method as it stands is its simplicity.

J. V. Hughes

Queen's University
Kingston, Ontario

[1] Han Kong-Chi, *Am. J. Physics* **16**, 398 (1948).
[2] J. H. Jeans, *The mathematical theory of magnetism and electricity* (Cambridge University Press, 1927), ed. 5, p. 611.

Simple Determination of Electronic Mass

The following approximate method for measuring the mass of the electron with the aid of a cathode-ray oscilloscope is much the simplest known to the writer. So far as he knows it has not hitherto been described.

The metal cover of the instrument is removed to get rid of magnetic shielding and to expose the tube to the inspection of the students and the manipulations of the demonstrator. The scope is oriented on the table so that the electron beam lies in the magnetic east-west line. The clamp screw holding the tube socket is loosened so that tube and socket may be rotated together about their axis of symmetry. The instrument used in trying out this method had lead wires to the socket long and flexible enough to allow the tube to be rotated to almost any position.

The beam is focused to a fine spot whose location is recorded by an ink dot placed directly on the end of the tube. Upon rotating the tube a few degrees it will be found that the spot is not carried around rigidly with the tube but is deflected by the earth's magnetic field to a new position on the screen. This new position is recorded by a second ink dot and the procedure is repeated until the orbit of the spot has been sufficiently determined. This orbit is an approximately circular loop whose diameter d is now measured.

It is easy to show that the electron mass m is approximately given by $B^2eL^4/2Vd^2$, where B is the strength of the earth's magnetic field, e the electronic charge, L the distance from anode to screen, and V the accelerating potential. The quantities B and e may well be taken from tables, but L and V must be measured. We measured L by breaking up a spare tube to get at the inner structure. In some tubes the anode may be seen through the glass, while in other cases the dimensioned drawings of the manufacturers may be called upon. Since L must be raised to the fourth power it should be determined with all practicable precision. The accelerating potential V may be measured directly with a vacuum-tube or other high-impedance voltmeter whose application has been found not to shift the spot.

The derivation will show that mass in grams results from the use of electromagnetic units for B, e and V, while L and d are expressed in centimeters. It is an advantage of this method that the full strength of the earth's field, rather than a component only, is utilized for the deflection. The precision to be expected may be judged from a recent demonstration using a Monarch oscilloscope with a 3BP1 scope tube. Two independent sets of data yielded results deviating respectively fifteen and five percent from the accepted electronic mass.

Paul Kirkpatrick

Bowdoin College
Brunswick, Maine

Three Primary Units Are Sufficient; a Reply

In his Letter to the Editor of the *American Journal of Physics*[1] and specifically his article in the *Journal of the Western Society of Engineers*,[2] Mr. Holm proposes that the theoretical advantages of the equations, $\mathbf{F}=e(\mathbf{E}+(\mathbf{v}/c)\times\mathbf{B})$, div $\mathbf{A}=-(1/c)\partial V/\partial t=-\partial V/\partial T$, and propagation in simple forms like $\nabla^2A-(1/c^2)\partial^2A/\partial t^2=\nabla^2A-\partial^2A/\partial T^2$, be carried over into the practical system. We should welcome this step toward uniformity were it not for the requirement that the speed of light c as well as the shape factor 4π and the 10^7 erg/joule conversion factor be included and *concealed* in a field constant k. For example, the magnetic force would be obtained by substituting $\mathbf{B}=k\int I'd\mathbf{l}'\times\mathbf{r}/4\pi r^3=(4\pi c/10^7)\int I'd\mathbf{l}\times\mathbf{r}/4\pi r^3$ in the term $e\mathbf{v}\times\mathbf{B}/c$ and cancelling c. Furthermore, if H is fictitious, as Mr. Holm agrees (H being the concentration of ampere-turns per meter which would be needed in a fictitious slender solenoid to produce the required value of B at any chosen point in the field), what can there be so fundamental about the field constant, $E/H=k=4\pi c/10^7=376.7$ ohms, as to justify its elevation to a "fourth fundamental unit?" In microwave guides, for example, H is properly regarded

as a convenient substitute for $B/\mu\mu_0$ in $\int Bdl = 4\pi\mu I/10^7$. The ratio of the two significant field quantities E and B is the propagation speed, (c meter/sec) = (E volt/meter)/(B weber/meter^2); alternatively one may use the dimensionless ratio $E/B = 1$, where $\mathbf{F} = e(\mathbf{E} + (\mathbf{v}/c) \times \mathbf{B})$ newtons and $\mathbf{B} = c\int I'd\mathbf{l}' \times \mathbf{r}/10^7 r^3 = 30\int I'd\mathbf{l}' \times \mathbf{r}/r^3$ volt/meter. In either case the ratio of the voltage across a parallel strip transmission guide to the current in the strip, $V/I = 4\pi c/10^7 = k$, follows directly from the ratio E/B without introducing any new kind of quantity, and provides the useful analogy to the impedance of the transmission line.

Here, once more, the supposed need for a fourth unit vanishes when attention is taken off an artificial field structure in which measurements are not made, and focused instead on the source and absorber where the measurements are made. Since k has the same dimensions as c, R has the dimensions L/T and thus the four-dimensional electrical basis VRLT^2 automatically reduces to a VLT three-dimensional system, both for the mks and for the "T" (ct) arrangement. Similarly the MLTQ form reduces automatically to three, QLT or ILT, in the manner indicated in my paper[3] since the force formula used includes only $k = 4\pi c/10^7$ as a field constant. As Mr. Holm indicates, the three-dimensional system further reduces to a two-dimensional system when time is defined in terms of a length. But it seems inadvisable to make the change to two dimensions unless or until all the theory of mechanics is also expressed in this two-dimensional system, because of the intimate relation of mechanical mass and internal electrical energy.

The arguments against action at a distance may be brushed off lightly, since they were early invoked to bolster a theory of a solid elastic ether by ridicule of the alternative theories. These discarded theories did not preclude action after the time required for propagation by means of a medium, but simply did not venture to postulate the means of transmission. The ether crashed on the Michelson-Morley experiment; and a return to action on electrons in the hands of Lorentz (B emerging as more fundamental than H), a partial return to relative motion according to Einstein, still more detailed action by and on electrical charges in the emission and absorption of photons, and a probability description of details of mutual action of electrons and protons have all assisted in anchoring field theory to its original base, electric charge, and to experiment. Dealing with the action by the electrical source and on the electrical absorber, where the experiments are performed, appears as the most safe and sound way to set up the basic definitions for quantities to be used in describing the field.

We may add here a correction in Table I, p. 437, of my November article.[3] The mks and emu dimensions of space permittivity in the ILT column, "$^*L^{-2}T^2$" were inadvertently omitted. Space permittivity of course has no meaning in the basic system.

University of Redlands, Redlands, California F. W. WARBURTON

[1] Gustave Holm, *Am. J. Physics* **17**, 168 (1949).
[2] Gustave Holm, *J. West. Soc. Engrs.* **53**, 87 (1948).
[3] F. W. Warburton, *Am. J. Physics* **16**, 435 (1948).

University of the Philippines, Manila

The Conference Board of Associated Research Councils calls attention to an opportunity for a visiting professor of physics to teach in the University of the Philippines, Manila, for one year beginning in July, 1949. The duties are to:

1. Conduct theoretical physics courses for juniors and seniors.
2. Conduct advanced theoretical courses for present members of the staff.
3. Aid in developing ways in which the study of physics may be made to implement projects of industrialization.

Arrangements are to be made under Public Law 584 (The Fulbright Act). Funds are available to cover maintenance and transportation outside the United States. It should be stressed that the only funds made available by the Act are in foreign currencies. It can be said, however, that stipends may be expected to bear a reasonable relation to the grantee's salary, and in addition an allowance may be provided for housing and cost-lf-living as well as a small allowance for books and equipment, local travel, etc.

For further details those interested should write to Francis A. Young, Assistant to the Executive Secretary, The Conference Board of Associated Research Councils, 2101 Constitution Avenue, Washington 25, D. C.

ANNOUNCEMENTS AND NEWS

Book Reviews

Microwaves and Radar Electronics. Ernest C. Pollard and Julian M. Sturtevant. Pp. 409, Figs. 195, $8\frac{1}{2}\times 5\frac{3}{4}$ in. John Wiley and Sons, Inc. New York, and Chapman and Hall, Ltd., London, 1948. Price $5.00.

This book deals with the techniques of microwave radar. Such a book is certainly of great interest to the electrical engineer and may well be of almost equal interest to the experimental physicist who contemplates using such techniques in his research. Since the book covers a great variety of material the treatment of certain subjects is severely compressed in many instances. This deficiency is somewhat relieved by the inclusion of a large number of references to original papers and reports. Unfortunately many of these references are to Radiation Laboratory Reports which may not be available at all institutions. Except in the first chapter on electromagnetic fields, use of mathematics is generally restricted to the statement of final results. However the qualitative discussions which have been substituted for mathematical developments may do more in giving the beginner an insight into what is going on than would a more elegant formal discussion.

The book *appears* to be divided into three sections. The first section consists of four chapters and deals with the generation, propagation, measurement and detection of high frequency electromagnetic waves. The following five chapters deal with low frequency circuits and components of the radar system. The last section consists of three chapters devoted to the application of microwave techniques to radar, communication and physical research. Physicists may find the chapter on applications to physical research a little disappointing as a consequence of the rather cursory way in which the many important developments in this field are treated.

In general the authors appear to have given an accurate and readable account of the technical aspects of radar. There are a few rather curious statements scattered throughout the book. For example, a footnote on p. 41 which deals with phase velocity in a waveguide contains the sentence, "Note that the phase velocity is not a velocity of propagation because it is not measured along the direction of energy flow."

Most of the chapters include a fair number of simple problems on which the reader may test his mastery of the material.

R. D. Spence
Michigan State College

Fourier Technique in X-Ray Organic Structure Analysis. A. D. Booth. Pp. 106+viii. Figs. 34, $5\frac{1}{2}\times 8\frac{1}{2}$ in. The Cambridge Series of Physical Chemistry; General Editor, E. K. Rideal. Cambridge University Press (The Macmillan Co., New York), 1948. Price $2.75.

In 1913, W. H. and W. L. Bragg announced the first crystal structure determination to be made by the methods of x-ray diffraction, a phenomenon which had been demonstrated in the preceding year by M. von Laue and his colleagues W. Friedrich and P. Knipping. Almost all the structure investigations which immediately followed these original announcements were confined to the simpler crystals of inorganic chemistry (unless we call diamond and graphite organic crystals). Although some workers did take pictures and make observations on organic crystals, no one seriously attempted the interpretation of such results. In 1921, W. H. Bragg read a paper before the Physical Society of London entitled "The Structure of Organic Crystals," in which he presented x-ray data on several such substances and included a preliminary account of the structure of anthracene and naphthalene. He based his analysis on the simple relationships which exist between the cell dimensions of these two compounds together with a qualitative consideration of the x-ray intensities. The exact determination of the structure of hexamethylene tetramine by Dickinson and Raymond and the long and bitter controversy over the structure of pentaerythritol were additional landmarks on the early course of organic structure analysis.

In his Bakerian lecture to the Royal Society in 1915, W. H. Bragg pointed out that the periodicity of a crystal made it possible to apply the Fourier analytic methods of Rayleigh to the study of x-ray diffraction problems. This idea lay fallow until 1925 when Duane called attention to it in setting up a quantum theory of x-ray diffraction. Duane, Havighurst and A. H. Compton then made application of such methods to the study of simple crystals and of the electron distributions of the atoms in them. It was the Fourier analysis of diopside by B. E. Warren and W. L. Bragg in 1928 that gave the final impulse to an already strong movement to develop Fourier methods for structure analytic work.

Since that time Fourier methods have been a guiding principle in almost all developments of x-ray analytical techniques. Although many of these techniques have other than Fourier analytic background, they have all been influenced by thinking in terms of Fourier procedures.

The fact that x-ray analysis determines the absolute value of the Fourier coefficients, and not the Fourier coefficients themselves makes it impossible in the general case to determine a crystal structure from x-ray data alone. In a few very important particular cases the presence of heavy atoms makes a direct determination of the structure possible as illustrated by the classical work of West on postassium dihydrogen phosphate and Robertson on the phthalocyanines. In general, however, the procedure is one of trial and error. A plausible structure is imagined, using a combination of chemistry, x-ray data, morphology, and physics. The calculated diffraction data for this structure are compared against the observed data for the crystal and in the light of this comparison the proposed structure is improved by a series of successive approximations.

Many methods have been developed with the purpose of making x-ray analysis a routine procedure and all of them

have decreased in one way or another the labor of the trial-and-error procedure. Only in a few special cases has the process of trial and error been removed entirely.

This long essay on crystal analysis is intended to place Dr. A. D. Booth's book in the background of the science for which it is written. During the past few years, Booth has devoted himself to the problem of minimizing the amount of trial and error in x-ray analysis and to the development of methods for carrying out the procedures of such analyses by modern computing methods. He has also made valuable contributions toward the solution of the error analysis problem for x-ray data.

The preface to his book indicates the purpose of the author in writing it: "The book is thus intended both for the research student, who has completed a course in the more elementary aspects of the subject and is contemplating his first structure determination, and for the advanced worker who, perhaps from some other field of crystallographic endeavour, comes to structure analysis after a period of absence." It is important to review the book in these terms.

In the course of about one hundred pages Booth covers the field professed by his title. He mentions almost every topic which would interest the persons referred to in the preface and gives an adequate set of references which would enable the reader to pursue each of these subjects further. The coverage is, however, very uneven. As is understandable, a detailed treatment is given of those methods and procedures with which the author has had experience and of those to which he has contributed. The discussion of the refinement of atomic coordinates, of methods for Fourier computation, and of the accuracy of atomic coordinates will be of considerable value to the mature worker in the field who is able to make his own critical evaluation of the methods proposed. The book will, however, mislead the persons mentioned in the preface who use it without other guidance. From it they will obtain a rather distorted view of the present approach to the analysis of complex structures. Booth is no doubt out of sympathy with this approach and so is the reviewer for that matter. Everyone rejoices in any method which will reduce the labor involved in the solution of crystal structure problems, and it is important that workers such as Booth extend themselves to the utmost in developing any and all possible labor saving devices. The unfortunate fact is, however, that crystal structures must be done now (as well as in the future) and the student who "is contemplating his first structure determination" must learn the methods which are now in use and are now ready for use. Booth unfortunately dismisses in few words a number of methods which are in successful use by dozens of workers in the field and spends many pages on devices which only presage bright things for the future. These pages will be inspiring to the mature worker, but of little use to an investigator with a structure problem in front of him in the spring of 1949.

There is little to criticize in detail in this book. As usual the Cambridge University Press has done an excellent job of book production and the reviewer has noticed only a few minor typographical errors and a few minor errors of fact. The first chapter on electromagnetic theory is somewhat sketchy and it is regrettable that the author did not seek to increase the generality of his arguments and to save space by the use of vector notation throughout the book.

The most important omission in the whole book is that of a discussion of the actual x-ray measurements and their relationship to the Fourier coefficients. The properties of natural crystals are such that this relationship is not the simple one indicated by the Rayleigh theory and the extensions of it which are expounded in this book. In the calculation of the Fourier coefficients from the intensities certain approximate corrections must be applied and the correct form for these approximations is difficult to determine. The resultant uncertainty in the determination of the Fourier coefficients is a constant source of difficulty even in the trial-and-error methods now in use and may prove even more important in the "automatic" methods with which Booth is concerned.

A. L. Patterson
Bryn Mawr College

Suggestions for Science Teachers in Devastated Countries. J. P. Stephenson. Pp. 88, 15.5×25 cm. United Nations Scientific and Cultural Organization, Paris, 1948. Gratis.

This remarkable little publication shows teachers who lack even elementary scientific equipment how to make useful apparatus from simple available materials. It is being distributed gratis by UNESCO to schools in Greece, Poland, Czechoslovakia, Austria, Hungary, Italy, China, and the Philippines.

The main section of the book is given over to the description of 201 experiments, of both discussion (or demonstration) type and student-performed, and detailed directions are given for constructing the apparatus and for carrying out the experiments, many of which are quantitative or semiquantitative. Helpful, clear line drawings are provided with each project. Their subjects are chosen from physics, astronomy, and chemistry, but a number of items of biological nature are included as well.

The author, science master at City of London School and member of the Royal Society Committee for Cooperation with UNESCO, has skilfully introduced ways of leading from some of the listed experiments into interesting by-paths as dictated by local facilities and interests. He has also added brief but useful chapters on the aims of science teaching, on visual aids, on materials and on laboratory techniques. The back cover of the booklet includes a table of logarithms, grids for curve plotting and even cutout millimeter rule, triangles, and protractor!

The average level of difficulty of the experiments seems to be about that of American junior high school science courses, although the reviewer feels that many experiments designed on this basis are equally valuable and instructive in connection with introductory courses at the college level. Mr. Stephenson reminds us, in his Introduction, that "these improvisations should not be thought of as makeshifts, and that they and the exercise of constructing them

are in the best tradition of science and science teaching. All the great scientists have used such apparatus and many have made their greatest discoveries in this way."

These facts seem to have been recognized only comparatively recently among science educators in this country, probably because of the ready availability of equipment and facilities in most of our schools—at least in comparison with the situation in many foreign countries. On the continent between the two wars, factory-made science equipment for instructional purposes was scarce and costly, and the "string and sealing wax school" grew of necessity. As a result, many useful and ingenious collections of experiments appeared, notably those of Hahn, of Rosenberg, and of Schwarz. Since that time the available material has been greatly augmented by several British and American contributions.

It is difficult to find fault with a publication as valuable and as timely as the present booklet, but one or two suggestions for improvement of its usefulness may be in order. It occurs to the reviewer that the visual aids and constructional materials referred to are exclusively British, and that apart from the regrettable omission of potentially useful materials of other origin, real confusion may result in countries where the corresponding American, French, or German products are better known. For that matter, why not go the whole way and distribute the book in translation in at least the main cultural languages?

IRA M. FREEMAN
Rutgers University

Physics for Arts and Sciences. L. GRANT HECTOR, H. S. LEIN, AND C. E. SCOUTEN. Pp. 731. The Blakiston Company, New York, 1948. Price $5.50.

Physics for Arts and Sciences, by Hector, Lein and Scouten, is intended for beginners and according to its preface makes use of "modern explanations" from the start, encourages "the student to think through particular types of problems by concrete reasoning rather than by an abstract mathematical approach" and features "the use of color in line drawings."

In order to use modern explanations, an all-too-brief attempt is made to acquaint the student with the atom; the Bohr picture is given but the experimental facts and the reasoning that gave rise to this picture are (quite understandably) lacking. There would be a definite gain if a clear distinction were made between fact and theory.

The end sought by "thinking through" problems is highly desirable. Some of the problems discussed in the text carry out this idea very nicely, but in others a few preliminary statements are merely followed by a formula. All of the problems worked in the text are good illustrations of the form in which the students should present their work: clear statements of reasons are given between the steps that lead to the solution.

The colored diagrams, usually in black and red, are very striking. How much they help the student can be determined only by actual use of the book.

The text touches upon the historical beginnings of all the sciences and ends with an account of the transmutation of elements. In the attempt to cover this field in 715 pages, it is evident that the treatment in the main must be descriptive and somewhat superficial. Accounts of key experiments that mark milestones in the development of the science have not been included. Apparatus has been given very little attention. An instructor who has the ordinary apparatus and desires to keep his laboratory in step with his class discussion may find more than the usual difficulty in doing so. "Experimental Problems" at the end of each chapter give very little help in preparing for large laboratory sections.

There is a sad confusion between mass and weight which appears again in the discussion of density and electrochemical equivalent. The entire subject of units is treated in a manner that throws an undue burden on the memory; as a rule a new formula is followed by a statement that if the quantities involved are expressed in such and such units, the "answer will come out in" such a unit. The metric and British systems of units are mentioned, but the former is identified with the cgs system and the latter is consistently called the English system throughout the first 274 pages and occasionally thereafter. A few other things that need attention are the distinction between action and reaction, the use of thermal capacity for the concept of thermal capacity per unit mass, and the diagram of the projection lantern. The chapter on illumination needs rewriting, using standard terms as defined, for example, in the *I.E.S. Lighting Handbook.*

The material in practically each chapter is well selected for students that need this type of course. Certain unusual teaching aids are provided: at the beginning of each chapter there is a short summary of the points emphasized; at the close are found "Some Important Facts" a "Generalization", "Questions and Problems" and "Experimental Problems." The problems are well selected and sufficiently numerous to allow different selections for different sections. The Blakiston Company is to be congratulated on the choice of paper and type, on the printing of diagrams and in fact on the entire format of the book.

W. WENIGER
Oregon State College

A Thought for Teachers

Often the rejected inventors and other misunderstood geniuses . . . come to him (Einstein) . . . *for Einstein's active and penetrating mind it has always been a pleasure to follow through a confused train of thought, to unravel it and to find the errors in it.*" (p. 295) "Einstein, His Life and Times" by PHILIPP FRANK. (Knopf, N. Y.)

New Members of the Association

The following persons have been made members or junior members (*J*) of the American Association of Physics Teachers since the publication of the preceding list [*Am. J. Physics* **17**, 221 (1949)].

Ahlstrom, Clarence W. (*J*), 3554 Chippewa Drive, Muskegon, Mich.
Albright, Charles Leonard, Apt. 102, 3509 Stuart Ave., Richmond, Va.
Brock, Robert Lewis, Physics Department, Oregon State College, Corvallis, Ore.
Chappelle, Austin Bemis (*J*), P.O. Box 1192, University of Arkansas, Fayetteville, Ark.
Daw, Harold Albert (*J*), 6310 S. Ninth E., Murray, Utah.
Dorn, W. Lee, 22 Grant St., Potsdam, N. Y.
Esterly, Henry Norcross, Kent State University, Kent, Ohio.
Evans, John P. (*J*), D-122 Lovell Rd., Houghton, Mich.
Gebhardt, George T. (*J*), Apt. 113, Hoff Heights, Santa Barbara, Calif.
Gibson, G. H., 41-32—58th St., Woodside, L. I., N. Y.
Goode, Paul J. (*J*), Douglass Houghton Hall, Houghton, Mich.
Haas, Robert D., 301 Douglas Ave., Kalamazoo, Mich.
Heer, Raymond R., Jr., 2208 Sherwood, Louisville, Ky.
Herman, Eugene B., 120 East Foster Ave., State College, Pa.
Hollis, Walter W. (*J*), 62 Willow St., Waltham 54, Mass.
Hungerford, Herbert Eugene, Jr., 14 Thomas Circle, Tuscaloosa, Ala.
Jaumot, Frank Edward, Jr. (*J*), 328 S. 52nd St., Philadelphia, Pa.
Kicher, John E. (*J*), 54 North Harrison Ave., Pittsburgh 2, Pa.
Lee, Kenneth V., 8 Ayleswade Rd., Salisbury, Wilts, England.
MacDonald, Frederick J. (*J*), 488 Baker St., West Roxbury 32, Mass.
Major, Schwab S., Jr. (*J*), 1711 North Vassar St., Wichita, Kans.
Man, Edward M., Jr., 230 Capen Blvd. Buffalo, N. Y.
Miller, Irwin (*J*), 659 Hawthorne St., Brooklyn, N. Y.
Monaghan, Floyd V., 805 Cherry Lane, East Lansing, Mich.
Moore, Wesley Eugene (*J*), 809 North Lightburne, Liberty, Mo.
Oliver, Anne Rebecca, State Teachers College, Albany, N. Y.
Poucher, Robert S., 43 N.E. 86th St., Portland 16, Ore.
Reynolds, Charles A., Sloane Physics Laboratory, Yale University, New Haven, Conn.
Sailor, Vance Lewis, 236-A Whitney Ave., New Haven 11, Conn.
Smith, Welborn H., Physics Department, University of Delaware, Newark, Del.
Steadman, Frank Morris, 2323 West Sixth St., Los Angeles, Calif.
Stewart, Albert, Antioch College, Yellow Springs, Ohio.
Taylor, Robert J. (*J*), Box 66, Tufts College, Medford, Mass.
Tenney, Gerold H., 704-47th St., Los Alamos, N. M.
Tobias, C. A., 1518 West Paterson St., Flint 4, Mich.
Trueblood, Frank S., 855 N. Vermont Ave., Los Angeles, Calif.
Vallese, Lucio Mario, Duquesne University, Pittsburgh 19, Pa.
Van Arkel, G. Harvey, 1010 Hoyt Ave., Apt. 9, Everett, Wash.
Ward, W. Harold, Tarkio College, Tarkio, Mo.
Webster, Robert Harold, Physics Department, Oregon State College, Corvallis, Ore.
Weissman, Simon A., 3373—12th Ave., Brooklyn 18, N. Y.
Williams, Charles S. (*J*), 509 East Fourth St., Alice, Tex.
Winter, Henry A., Gogebic Junior College, Ironwood, Mich.
Womaski, Anthony Joseph, 1226—12th St., Oshkosh, Wisc.
Worth, Donald Calhoun, Sloane Physics Laboratory, Yale University, New Haven, Conn.
Zarnowitz, Richard M. (*J*), 3974 Fleet St., San Diego 10, Calif.

Dynamical Double-Talk

The following modification of a familiar problem in elementary kinetic theory is presented for its possible cosmic implications.

Consider a surface of area S which is bombarded by a stream of perfectly elastic particles of unit mass. Let the number of particles per unit volume in the stream be N, and let the component of their velocity normal to the surface be U. Our problem is to find the total momentum transferred to the surface in time T.

The momentum transfer by each particle will be 2U. The particles that strike the surface in time T will be those lying in a volume UTS. The final result is, then: NUTS 2U.—E. SCOTT BARR, *University of Alabama.*

DIGEST OF PERIODICAL LITERATURE

New International Temperature Scale

On January 1, 1949, the National Bureau of Standards began using the definitions of the International Temperature Scale of 1948 both in its own research program and in calibrating instruments for other scientific and industrial purposes. Based on a draft prepared by members of the Bureau staff, the new scale was adopted at Paris by the Ninth General Conference on Weights and Measures in October 1948, and the official text was approved for publication before the end of the year. This is the first revision of the International Temperature Scale since its adoption 21 years ago. The experimental procedures by which the scale is to be realized are substantially unchanged; but certain refinements, based upon experience, have been incorporated to make the scale more uniform and reproducible.

The experimental difficulties inherent in the measurement of temperature on the thermodynamic scale (a scale based on energy changes in a Carnot cycle) led to the establishment in 1927 of the practical scale known as the International Temperature Scale. This scale is based upon six reproducible equilibrium temperatures, or *fixed points*, to which numerical values are assigned, and upon specified interpolation formulas relating temperature to the indications of specified standard temperature-measuring instruments. The scale is designed to conform, as nearly as practicable, to the thermodynamic Celsius[1] scale as it is now known. At the present time, however, it is possible to obtain values of temperature on the International Temperature Scale more accurately than on any thermodynamic scale.

The International Temperature Scale of 1927 proved useful in providing a stable, uniform, and precise basis for obtaining temperatures. However, since the adoption of this scale, the increasing precision attained in temperature measurements had made it apparent that some revision was desirable in order that measurements of physical constants—for example, the freezing and boiling points of pure compounds—might be made on a more exactly comparable basis by laboratories in all parts of the world. Because of the present leadership of the United States in the fields of heat and thermometry, the major responsibility for proposing and obtaining agreement on the changes fell to the National Bureau of Standards. After many consultations with scientists and laboratories in this country and abroad, the Bureau prepared a draft which formed the basis of the document finally adopted by the General Conference as the International Temperature Scale of 1948.

The six fixed points of the 1927 scale were the boiling point of oxygen (−182.97°C), the freezing and boiling points of water, the boiling point of sulfur (+444.60°C), the melting point of silver (+960.5°C), and the melting point of gold (+1063°C). From −190° to +660°C, the measure of temperature was based on the indications of a standard platinum-resistance thermometer used in accordance with specified formulas. From +660°C to the gold point, a platinum-platinum rhodium thermocouple was the reference instrument; and above the gold point, the optical pyrometer has been standard.

The same fixed points, with one slight modification, are specified in the 1948 scale, and the laboratory procedures for obtaining temperatures between fixed points are essentially the same as those previously used. Only two revisions in the definition of the scale result in appreciable changes in the numerical values assigned to measured temperatures. One of these is the change in the value for the silver point from 960.5° to 960.8°C, which affects temperatures measured with the standard thermocouple. Thus, in the range between 630° and 1063°C, numerical values of temperature are higher than on the 1927 scale, the maximum difference being about 0.4 degree near 800°C. The adoption of a new value (1.438 cm deg) for the constant c_2 in the radiation formulas changes all temperatures above the gold point. In the new scale, Planck's radiation formula is specified instead of Wien's for calculating temperatures above the gold point as observed with an optical pyrometer. Since Planck's law is consistent with the thermodynamic scale even at high temperatures, this change removes the upper limit to the scale formerly imposed by the use of Wien's law.

There are several other important modifications in the scale which cause little or no change in numerical values for temperatures but serve to make the temperatures more definite and reproducible. For example, the standard platinum resistance thermometer is to be used as a reference instrument from the oxygen point to the freezing point of antimony (about 630°C), rather than over the range from −190° to +660°C. Platinum of higher purity is also specified for the standard resistance thermometer and standard thermocouple, and smaller permissible limits are given for the electromotive force of the standard thermocouple at the gold point.

[1] The Ninth General Conference decided to abandon the designation "centigrade" and use "Celsius" instead.